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THE IBERIAN STRUCTURAL AXIS

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ABSTRACT

The Iberian structural axis strikes approximately northwest-southeast through Morgan City and Lafayette, and presumably extends northwesterly past Pine Prairie to connect with the north Louisiana geosyncline. The existence of the structural axis is indicated by the plan of the depth to the top of the salt domes. Certain physiographic data suggest arching of the surface along the Iberian structural axis. The axis is assumed to represent a geosyncline trough. The arching is presumed to be the effect of less compaction and greater uplift of the salt in the area of shallow domes than in areas of deep domes, and may be partially responsible for the form of the land area of the Mississippi Delta in the Terrebonne Parish area and for the eastward deflection of the Mississippi River. There is a suggestion that the axis may bend northeastward, possibly to connect with the Appalachian geosyncline.

The presence of some sort of structural feature striking northwest-southeast through Lafayette in Lafayette Parish, and through Morgan City in St. Mary Parish, is indicated by the plan of the depth to the top of the lime rock—gypsum—anhydrite—salt core of the salt domes in southern Louisiana and is suggested by several physiographic features; or rather, the assumption of slight arching of the surface over the crest of the writer's Iberian zone of shallow salt domes suggestively explains the southeastward swing of the lower Mississippi River, the anomalous course of Bayou Teche—Bayou Boeuf, the Iberian Peninsula of dry land, and the form of the southwestern edge of the Mississippi Delta, and thereby attains a modest probability. Such slight doming is rather easily explained by the lesser compaction of sediments and the greater uplift of the salt in

the areas of shallow salt domes. Comparison of the plan of the depth to the top of the domes of southern Louisiana with the plan of the occurrence of the domes in northern Louisiana suggests that the Iberian structural feature is a large synclinal trough. This structural feature provides some slight data for further speculation.

The existence of an Iberian structural feature has been advocated orally by the writer for many years. Until recently the structural feature was assumed to be an anticlinal arch. The physiographic and stratigraphic data on which his argument for the presence of an anticlinal arch was based, did not satisfy him; and he did not publish his interpretations. His postulation of it, however, is mentioned by Stephenson.[1] The supposed stratigraphic data now are known to have been based on erroneous correlation; and on the basis of the better maps and data now available much of the earlier physiographic argument is untenable. But new information definitely indicates the existence of the structural feature.

The definite evidence of the Iberian structural axis is given by the long narrow zone of extremely shallow salt domes which strikes northwest-southeast through Pine Prairie (the northernmost dome in Figure 1) and southeastward past Lafayette, Jeanerette, and New Iberia. Only ten of the fifty known domes in southern Louisiana rise within 500 feet of the surface. Seven of those shallow domes lie on the Five Islands salt-dome line, which forms the central axis of that zone of shallow domes. The Five Islands are the four domes in a line, southwest of New Iberia and Jeanerette, and a fifth dome, Belle Isle, southwest of Morgan City. Those five domes have the shallowest salt cores of all the domes of the Gulf Coast of Texas and Louisiana. On Avery and Weeks Island, the salt rises above the general level of the surrounding country; and on Jefferson Island and Belle Isle, it rises within 50 feet of the general level of the surrounding country. Pine Prairie, at the north end of the zone of shallow domes, is one of the two domes of the coastal group on which the cap rock is exposed at the surface. The other is Gyp Hill (Falfurrias), Brooks County, in south Texas.

The depth of the domes can be seen from Figure 1 to increase away from the Iberian zone of shallow domes westward, southwest-

[1] L. W. Stephenson, *Geol. Soc. Amer. Bull.*, Vol. XXXIX, pp. 887–900.

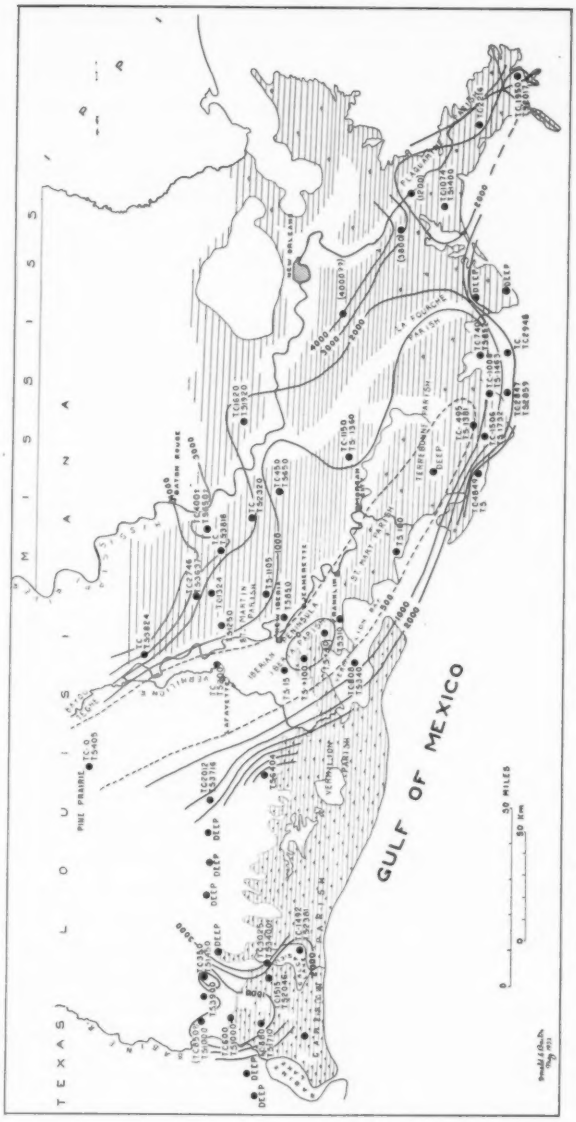

FIG. 1.—Map of the depth to the top of the salt-cap core of the salt domes of southeast Louisiana

ward, southward, eastward, and northeastward. On the west, there is a broad area of very deep domes: Gueydan with the top of the dome (salt) at 6,404 feet; Roanoke, Welsh, Iowa, and Lockport with the salt so deep that it is known only from the results ot geophysical work. It is interesting to see that the plan of the depth of the domes of Figure 1 corroborates the failure of the seismic method to find any domes in the broad coastal area south of the first three of those four domes, although on the east edge of that area, the seismic method led to the discovery of Gueydan with the top of the dome (salt) at 6,404 feet. The Iberian zone of shallow domes, therefore, is a very definite feature.

Only two domes—Choctaw (T. C. 400±, T. S. 650±) near Baton Rouge, and Minor (Deep) within the 500-foot contour south of Morgan City—do not seem to fit into the otherwise rather simple plan of the arrangement of the depth to the top of the dome in southern Louisiana.

A cross northeast-southwest trend seems to be manifest halfway between Baton Rouge and New Orleans. The zone of shallow domes seems to bulge northeastward halfway between New Orleans and Baton Rouge. The depth to the top of the cap at Napoleonville (T. C. 450, T. S. 650) is only 450 feet; and the depth to the top of the cap at Sorrento (T. C. 1,620, T. S. 1,920) 1,620 feet, is less than it should be, if the Iberian zone of shallow domes had a smoothly curving northeast flank. A parallel trend of extra deep domes is delineated immediately to the southeast by Barataria (4,000?), Chacahoula (T. C. 1,150, T. S. 1,360), and Minor (Deep) domes.

The Five Islands[2] salt-dome line is one of the two most definite alinements of domes in the Gulf Coast; its trend delineates the trend of the Iberian structural axis; and it approximately marks the axis of the Iberian zone of shallow domes. The other most definite alinement of domes is the line of domes which parallels the Southern Pacific main line from Lafayette west. The attempted alinement of the domes of the Gulf Coast area has long been an indoor sport of geologists. The alinement of the Five Islands, however, is definitely evident. The five domes, furthermore, show a common family resemblance and are more like each other than they are like any other

[2] "Island" = high dry land rising out of the marsh or lowlands.

domes of the Gulf Coast. They indicate a line which is slightly concave northeastward. Extrapolation of such a line, of course, is uncertain; but the northward extension of the line must pass near Pine Prairie dome; and the southeastward extension of the line will pass not far from the Four Isle dome (T. C. 495, T. S. 1,381). The Bay St. Elaine dome (T. C. 1,008, T. S. 1,463) probably would seem to belong also on the Five Islands line, but the farther extension of the line eastward or southeastward is uncertain. It may continue southeastward through the Caillou Island dome (T. C. 2,948) or bend more sharply eastward through the Lake Barre (T. C. 740, T. S. 852) and Leesville (Deep) domes.

The physiographic argument for the existence of the Iberian structural axis is based on the simultaneous explanation of: (*a*) the Iberian Peninsula of dry land (in part); (*b*) the anomalous course of Bayou Teche–Bayou Boeuf; (*c*) the form of the southwestern edge of the Mississippi Delta; and (*d*) the southeastward swing of the Lower Mississippi River by the assumption of slight arching of the surface over the Iberian zone of shallow domes. That assumed slight arching is explained easily by the lesser compaction of the sediments and by the greater uplift of the salt in areas of shallow domes than in areas of deep domes. That simultaneous explanation of several physiographic features and the likelihood of the lessened compaction of sediments in the Iberian shallow zone lend strong presumption in favor of the argument. The argument is as follows:

A peninsula of dry land, termed by the writer the "Iberian peninsula," extends from New Iberia past Morgan City. The swampy back bottoms of the Mississippi River border it on the northeast, and the salt marshes border it on the southwest. In southwest Louisiana, the contact between the dry land to the north and the coastal marshes to the south follows a serrately straight course which strikes slightly south of east across southwest Louisiana (see Fig. 2). If it had not bent southeastward down the southwest flank of the Iberian Peninsula of dry land, the contact between the coastal marsh and the dry land would pass through, or very slightly to the south of, New Iberia.

The southeastern half or two-thirds of the present peninsula is merely the crest of the natural levees of Bayou Teche–Bayou Boeuf.

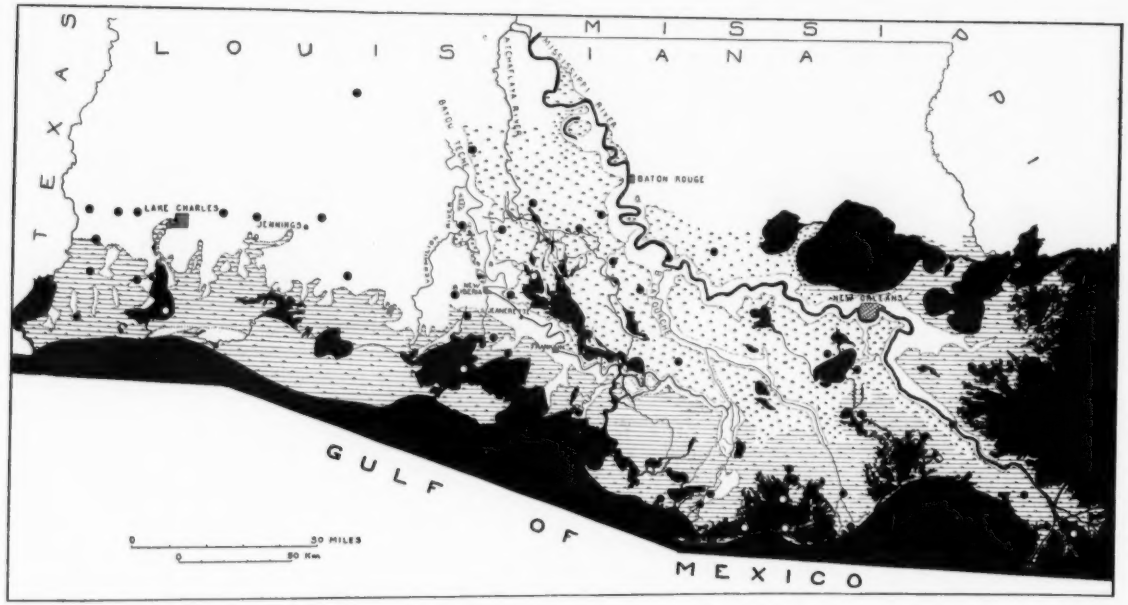

FIG. 2.—Sketch map of southern Louisiana, showing the relations of water, marsh, and dry land

From a part-way point between Jeanerette and Franklin southeastward, the Teche has much larger meanders than in the reach between that part-way point and New Iberia. Below that part-way point, the strip of dry land more or less follows the sinuosity of the Teche; the width of the belt of dry land on the left bank of the Teche is the same as that on the right bank; and the height of the crest of the ridge on the southwest is the same as that on the northeast. The present strip of dry land from Franklin southeastward seems clearly to be the upper part of the levees of the Teche.

The Iberian Peninsula, northwest of the large meanders of Bayou Teche a few miles upstream from the town of Franklin, quite definitely is not the right levee of Bayou Teche, but is a peninsula of older terrane. The evidence is drawn both from the physiography and from the soils. The surface of the Iberian Peninsula from Lafayette to New Iberia is an upland level which lies above the upper bottoms level on which Bayou Teche flows. The two levels are separated by an eastward-facing scarp which rises 20–25 feet above the marshes and lakes at its foot. The southwest extension of the scarp can be recognized for most of the distance between New Iberia and Jeanerette. The levee of the Teche slopes gently southwestward into the Grand Marais.[3] The scarp rises abruptly out of the Grand Marais and forms its southwest border, but in the immediate vicinity of New Iberia and Jeanerette both the scarp and the Grand Marais are indefinite. The scarp for most of the length of the Grand Marais has a relief of 7–10 feet. The Grand Marais, as its name signifies, is a broad enclosed marshy area (its soils were mapped by the United States Bureau of Soils as peat), and it quite evidently is the southeastward extension of the swamps which extend northwestward from New Iberia between the scarp and the right levee of Bayou Teche. The scarp seemingly is the western valley scarp of the Mississippi Valley.

The soil map of Iberia Parish[4] also shows that the upland behind the scarp is not part of the levee of Bayou Teche. The soils of the levees of the Teche in Iberia Parish were mapped as predominantly the Olivier silt-clay loam and the Iberia silt-clay loam, although

[3] Great enclosed marsh.

[4] U.S. Bureau of Soils and Chemistry, *Soil Survey of Iberia Parish, La.*

patches of the Olivier silt loam were mapped (see Fig. 3). Those two silt-loam soils form two rather narrow bands on the soil map, one on each side of Bayou Teche. The area from the scarp along Grand Marais southwest to the edge of the coastal marshes was mapped as having predominantly Iberia silt-loam soil, except in the northwest corner of the parish, where the soil is the Olivier silt loam. Locally, there are patches of Iberia silt-clay loam. The Iberia silt-loam soil forms a belt, 4–7 miles wide, which extends from New Iberia west to the Vermilion Parish line, a distance of 10 miles, and probably yet farther westward into Vermilion Parish; and which extends from New Iberia southeast to the St. Mary Parish line and probably yet farther southeastward into St. Mary Parish. Northwest of New Iberia, the belt of Iberia silt-loam soil is on the definite upland back of the scarp from New Iberia to Lafayette. Silt-loam soils are sandier than silt-clay-loam soils. Where both silt-loam soils and silt-clay-loam soils are present on a natural levee, the silt-loam soils normally cover the crest and upper third of the flank of the levee, and the silt-clay loams cover the middle and lower portion of the flanks and grade into clay soils at the foot of the levee; for in the formation of natural levees, most of the sand is deposited immediately outside the channel. The Iberia and Olivier silt-loam soils should mantle the crests and upper portions of the levees; and the Iberia and Olivier silt-clay-loam soils should mantle the middle and lower portions of the levees, if those silt-loam soils belonged to soils of the Bayou Teche levees. The extension of a moderately homogeneous belt of Iberian silt-loam soil up on the upland northwest of New Iberia and westward from New Iberia into Vermilion Parish also indicates that this belt has no connection with Bayou Teche.

The evidence of the physiography and of the soils shows, therefore, the presence of an Iberian Peninsula of upland paralleling the right levee of Bayou Teche southeastward into St. Mary Parish.

Bayou Teche–Bayou Boeuf has an anomalous course. From New Iberia to Morgan City, a distance of 60 miles, Bayou Teche flows parallel to the coast. Bayou Boeuf clearly is the former continuation of Bayou Teche; the channels and levees of the two bayous are of the same size; the meanders of Bayou Boeuf continue those of Bayou Teche and are of the same size and pattern. The Atchafalaya River

FIG. 3.—Sketch map of the Bayou Teche area (after U.S. Bureau of Soils, soil survey maps of St. Martin and Iberia parishes and after the U.S. War Department topographic sheets).

below Morgan City has small low natural levees compared to those of Bayou Teche–Bayou Boeuf. The natural levees of Bayou Boeuf are continuous with those of Bayou Teche except as the Atchafalaya River separates them. The lower Atchafalaya River seemingly is younger than the Bayou Teche–Bayou Boeuf system; its waters became ponded behind the natural levees of the latter system, but ultimately breached them. The Atchafalaya River then took a direct course across the Bayou Teche–Bayou Boeuf system to the Gulf and captured both Bayou Teche and Bayou Boeuf. The original Teche, therefore, continued its anomalous course parallel to the coast southeastward from Morgan City for 30–40 miles; and in all it flowed for almost 100 miles parallel to, and not far from, the coast instead of taking a direct course to the Gulf.

The course of the Teche between New Iberia and Jeanerette clearly was controlled by the scarp which faces eastward over the Grand Marais and which presumably was the western valley scarp of an early Mississippi valley. The Teche above Jeanerette and New Iberia is flowing in the bottomlands below the upland level of Lafayette and Iberia parishes. It impinges on the valley scarp at New Iberia and takes a straight course at the foot of the scarp from New Iberia to some place between Jeanerette and Franklin.

The anomalous course of the Teche from that place southeastward for 60 miles must have been controlled by some linear topographic feature which kept the Teche from taking a direct course to the Gulf, and which forced it to continue its southeastward course. A northeastward facing scarp at a depth of 50–70 feet was indicated by a torsion balance survey near Patterson. It would seem rather probable that the scarp which faces northeastward over Spanish Lake and the Grand Marais originally must have continued southeastward past Patterson and Morgan City and must still be in existence, buried under more recent sediments. Somewhat the following physiographic history would seem to be indicated: In very late Pleistocene or early post-Pleistocene times, the upland surface in Lafayette and Iberia parishes extended southeastward over what would essentially be the area within the 500-foot contour of the depth-to-the-dome map, Figure 1. The Mississippi Valley of that time was cut back into that upland until the right valley scarp lay approximately

where the base of the right levee of Bayou Teche–Bayou Boeuf now lies. The original Teche was formed for some cause or other which is not evident from the studies of this area and which produced the diversion of large quantities of water down the back bottoms of that ancient Mississippi Valley. From New Iberia southeastward, the original Teche took a course along the foot of the valley scarp. Gradual progressive subsidence submerged the southeastward half or two-thirds of the ridge of upland, of the scarp, and of that ancient Mississippi Valley under the accumulating sediments of the Mississippi Delta. It is perhaps possible that the subsidence was not uniform over the whole area, but increased progressively southeastward from Franklin. The Teche built up its levees concomitantly with the subsidence and, thereby, was able to maintain its course. All trace of the scarp and upland at the surface is now gone southeast of Franklin. Measured by the probable width of its channel and by the width from levee crest to levee crest, the ancient Teche was about one-third as wide as the present Mississippi River between Baton Rouge and New Orleans and must have carried approximately one-ninth as much water as the present Mississippi River. The size of the natural levees shows that it was carrying a considerable load of sediment. Some later happening caused the diversion of most of the water from the Teche. Some other, or perhaps the same, happening in more recent post-Pleistocene times, caused the diversion of the Atchafalaya waters down the thalweg of the bottom lands between the Mississippi levee and the Teche levee. The waters were impounded behind the left levee of the Teche-Boeuf and perhaps also behind the levees of the Bayou La Fourche distributaries which come down from the north to meet Bayou Boeuf near Houma. The waters finally broke across the Teche-Boeuf levees at Morgan City and established the present lower Atchafalaya River.

The extension of that ancient peninsula of upland so much farther gulfward than the present upland may be explained by two alternative hypotheses of approximately equal probability. According to the first, the western edge of a very late Pleistocene or very early post-Pleistocene delta must have had much the same extent as the present land, lake, and marsh area of Iberia, St. Mary's, Terrebonne, and La Fourche parishes, but must have stood a little higher above

sea-level than the present level of the land. The eastern flank of that ancient Iberian peninsula then would have been the west valley scarp of an early post-Pleistocene or very late Pleistocene Mississippi valley which was incised in that delta. Positive evidence of that ancient delta is not available. The eastern half of Lafayette Parish and the part of Vermilion Parish well north of Abbeville are a grand patchwork of series upon series of majestic ancient meanders of a river considerably larger than the present Mississippi River. These old meanders are revealed by slight differences in the soil, vegetation, and topography. They were mapped to a slight extent by the United States Bureau of Soils in the soil map of Lafayette Parish, but stand out much more clearly in the aërial photographic maps of the area. The delta of that ancient Mississippi River must have lain yet farther to the south and now must lie under the waters and marshes of southern Vermilion and Iberia parishes and under the waters of the Gulf. That ancient meander system does not extend into Iberia Parish. According to Howe, (*a*) the Pensacola terrace is represented in this region by the belt of Iberia silt-loam soils, which are the soils of the upland of the Iberian Peninsula in the parish, and (*b*) the Pensacola terrace truncates the area of that series of ancient meanders. If the ancient Iberian Peninsula which extends so far southeast were part of an ancient delta, the latter must have been younger than the delta of the Mississippi which produced the meanders of Lafayette and Vermilion parishes.

According to the second alternative hypothesis, there was faint arching of the surface along the Iberian structural axis. Incision of the early post-Pleistocene Mississippi Valley cut back the northeast flank of the topographic arch to the scarp which faces the Grand Marais and which presumably extended southeastward through Patterson and Morgan City.

Faint arching along the Iberian structural axis is expectable. The stratigraphic section in the New Iberia–Morgan City area consists presumably of 20,000–30,000 feet of unconsolidated sediments. The post-Miocene sediments alone seem to have a thickness of 5,000–6,000 feet. Calculations in connection with torsion-balance surveys of salt domes in the Gulf Coast and studies of the variation of seismic velocities with depth both indicate increasing compaction of the sedi-

ments with depth. Progressive compacting of the sediments seemingly has been, and must be, taking place. The shallow salt domes along the Iberian structural axis reach to, or almost to, the surface. Mechanically the salt cores should act as supporting pillars and should retard compaction of the sediments which they pierce. Compaction of the sediments, therefore, should be less in an area of shallow domes than in an area of moderately deep domes, and less in the latter area than in an area of very deep domes or in an area of no domes. It is perhaps more than a chance coincidence that Galveston Bay lies in an area of very deep domes; that the Neches and Sabine rivers, and Sabine Lake, into which they flow, lie in the next deep dome area to the east; and that Calcasieu River and Calcasieu Lake lie on the edge of the next deep dome area yet farther to the east. The general arching over the area of shallow domes might also be explained in part by the greater active recent uplift of the salt cores in the shallow domes. The high salt-dome mounds, the lack of marine cliffing of the mounds, and the position of the salt near or above the general surface level suggest very recent uplift in the domes of the Five Islands. The same movement of uplift may have affected all the domes of the Iberian zone of shallow domes; and an effect of the general uplift of the domes might be a slight general arching of the whole area of shallow domes. Arching of the surface, therefore, is an entirely plausible explanation of that ancient Iberian Peninsula of upland, which extended so far to the southeastward.

Arching of the surface along the Iberian structural axis will explain also the rather anomalous course of the Lower Mississippi River and the position of the edge of the land over the southwestern part of the Mississippi Delta.

The Mississippi River in southern Louisiana rather anomalously bends consistently to the left (except below New Orleans) instead of taking a direct course to the Gulf through the Morgan City area. The ancient Mississippi River which carved that early post-Pleistocene valley into the upland of the ancient Iberian Peninsula likewise must have had the same anomalous trend; or, at least, the course of the Bayou Teche–Bayou Boeuf indicates that the west scarp of that ancient valley must have had that trend. Comparison of the course of the Mississippi River in southern Louisiana with the position of the

northeastern and eastern flank of the Iberian zone of shallow salt domes shows that the Mississippi River starts to bend eastward where it begins to impinge on the flank of that Iberian shallow zone and then that, in its further course, it coasts along the edge of that zone.

The edge of the land in Iberia, St. Mary, Terrebonne, and La Fourche parishes follows fairly closely the edge of the Iberian shallow zone. The edge of the land bends rather sharply around the southeastern end of the zone into a northeast direction where, as it were, the Iberian shallow zone begins to plunge rapidly in a southeastward direction. The position of the edge of the land could be explained by normal deltaic activity. But the postulated arching of the surface along the Iberian structural axis would explain equally well the general position of the edge of the land in that area.

Arching of the surface along the Iberian structural axis is not indicated conclusively by the physiographic evidence. But the reasonable expectability of the arching, the coincidence of the ancient Iberian Peninsula of dry land with the Iberian structural axis, and the explanation of these several physiographic features by the assumption of arching of the surface over the Iberian shallow zone raise a moderate degree of presumption in favor of that assumption.

Faint arching of the surface does not necessarily mean that there has been much structural arching in the subsurface.

Comparison of the Iberian zone of shallow domes with a depth-to-the-dome map of the salt domes of north Louisiana suggests (*a*) that both areas are part of the same structural feature, and (*b*) that, therefore, the Iberian zone of shallow domes reflects the presence of a geosyncline at very great depth. The salt domes of north Louisiana lie in a slightly elongated area whose major axis strikes approximately northwest-southeast. The axis of the north Louisiana salt-dome area and the axis of the Iberian zone of shallow domes, therefore, are essentially parallel (Fig. 4) and are very nearly on the prolongation of each other; and the distance between the northernmost dome in the Iberian zone of shallow domes and the southernmost of the north Louisiana domes is short. It looks, therefore, as if the north Louisiana domes and the Iberian zone of shallow domes are parts of

the same structural feature. The north Louisiana domes lie in a small geosyncline between the Sabine uplift to the west and the Monroe uplift to the east. The north Louisiana geosyncline is only faintly reflected in the Tertiary and uppermost Cretaceous beds, and, in that, it is in strong contrast to the East Texas geosyncline, but it is strongly reflected in the Lower Cretaceous formations. If the north Louisiana salt domes and the Iberian zone of shallow salt domes are parts of the same structural feature, the Iberian structural axis must

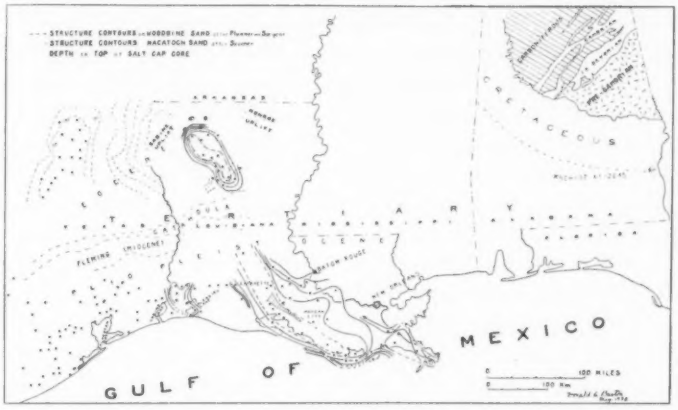

FIG. 4.—Sketch map of east Texas, Louisiana, Mississippi, and Alabama, showing the relation of the Louisiana salt domes to the main surrounding structural features.

represent a geosyncline in beds at very great depth—at least 20,000 feet and perhaps more than 30,000 feet below the surface. The geosyncline may have been in existence during the deposition of the salt series or may have been formed after its deposition. The Magdeburg-Halberstadt salt basin in Germany, for example, was formed after the deposition of the Zechstein salt series.

Greater thickness of the mother salt series within the synclinal trough than on the flanks of the trough or in the area outside the trough presumably explains the shallowness of the salt domes along the Iberian structural axis compared to the greater depth of the salt domes off the axis.

Further, probably risky, speculation tentatively suggests the possibility of a connection of the Iberian geosyncline with the Appalachian geosyncline. The Iberian zone of shallow domes shows a distinct northeastward bulge halfway between Baton Rouge and New Orleans. Where the folded Appalachians of the old Appalachian geosyncline dip under the Gulf Coastal Plain, they are headed approximately for New Orleans. Some geophysical work in Mississippi is reputed to suggest that the Appalachian folding continues well into Mississippi without essential change of strike. If the Appalachian geosyncline continues across Mississippi with little change of strike, it should pass between New Orleans and Baton Rouge and approximately connect with the northeastward bulge of the Iberian zone of shallow domes between Baton Rouge and New Orleans. The pendulum observations of the United States Coast and Geodetic Survey show an increase in anomalous gravity from New Orleans to the mouth of the Mississippi. Scattered torsion balance surveys in the marshes and delta area south of New Orleans show a southeasterly to southerly regional gradient in that area. The positive anomaly over the lower delta may indicate that the Appalachians extend south-southwestward across the tip of the delta and that in the marsh area south of New Orleans the buried northwest flank of the Appalachians is dipping northwestward into a geosyncline.

The suggestion, therefore, arises that the Appalachian geosyncline runs into the Iberian geosyncline at the northeastward bulge of the Iberian zone of shallow salt domes and that the Iberian–north Louisiana geosyncline is the northwest extension of the Appalachian geosyncline.

In delineating the orographic framework of the United States, many geologists display an inclination to bend the Appalachians northwestward across northern Mississippi into Arkansas. This suggestion of the possible connection of the Iberian–north Louisiana geosyncline with the Appalachian geosyncline is made to provide them with an alternative course for the latter. The scanty geologic and geophysical evidence available would seem to the writer to suggest that the Appalachians continue their southwestward course into the New Orleans area, but that does not necessarily indicate any

genetic connection between the Appalachian geosyncline and an Iberian–north Louisiana geosyncline. The former might be much older than the latter, and the cross trend in the depth of the depth-to-the-top-of-the-dome halfway between Baton Rouge and New Orleans might be reflection of the older structure superimposed upon the younger.

The question of the possibility of an early Paleozoic age of the salt of the Louisiana salt domes arises as another somewhat far-fetched speculation. Early Paleozoic salt extends down the Appalachian geosyncline at least as far as Saltville, Virginia. If the Iberian–north Louisiana synclinal trough is the continuation of the Appalachian geosyncline, the salt of the Louisiana salt domes might be correlated with that early Paleozoic salt of the Appalachian geosyncline, and that correlation would perhaps be no farther fetched than the more common correlation of the salt of the Gulf Coastal Plain with the Permian salt of west Texas.

Certain facts, which are indubitable, and certain conclusions and speculations, which have varying degrees of probability, have been presented in this paper. The very striking arrangement of the depth to the top of the salt domes would seem clearly to reflect structure in the subsurface. The correlation of the Iberian zone of shallow domes with the salt dome area of north Louisiana and the corollary that the Iberian structural axis is a geosyncline at great depth would seem to have a fair degree of probability but not to be proved conclusively. Arching of late Pleistocene to Recent date along the Iberian structural axis would seem to have a reasonable degree of possibility as an explanation of the Iberian (topographic) peninsula and to explain a series of features, such as the anomalous course of Bayou Teche–Bayou Boeuf–Bayou Black, the position of the edge of the land in St. Mary, Terrebonne, and La Fourche parishes, and the eastward bending of the Mississippi River below Baton Rouge; but yet the existence of the arching has not been proved conclusively. The writer would like very much to see corroboratory subsurface evidence. The correlation of the Iberian geosyncline with the Appalachian geosyncline is a tentative hypothesis which should be tested further. Geophysical work by oil companies in Mississippi within the next few

years will probably prove or disprove the hypothesis. The correlation of the Louisiana salt series with the early Paleozoic salt series of the Appalachian geosyncline is merely a suggestion which on the present data has merely a small degree of possibility.[5]

[5] A very detailed annotated bibliography of the geological literature of the area and much discussion of the geology of the area is contained in *La. Dept. of Cons. Geol. Bull. No. 1* (1931), "Geology of Iberia Parish," by H. V. Howe and C. K. Moresi.

Soil Survey maps by the Bureau of Soils and Chemistry, U.S. Department of Agriculture: C. J. Mann and L. A. Kolbe, Soil Survey of Iberia Parish, Louisiana, Field Operations of the Bureau of Soils, 1911, 1914; A. H. Meyer and N. M. Kirk, Soil Survey of Lafayette Parish, Louisiana, 1916; A. H. Meyer and B. H. Hendrickson, Soil Survey of St. Martin Parish, Louisiana, 1919.

Topographic maps: Corps of Engineers, U.S. Army, Second New Orleans District; Arnaudville Quadrangle, No. I–31; St. Martinsville Quadrangle, No. I–32; Derouen Quadrangle, No. I–33; Osca Bayou Quadrangle, No. J–31; Loreauville Quadrangle, No. J–32; Jeanerette Quadrangle, No. J–33; Bayou Sale Quadrangle, No. J–34; Chicot Lake Quadrangle, No. K–32; Foster Quadrangle, No. K–33; Belle Isle Quadrangle, No. K–34; Pointe au Fer Quadrangle, No. K–35; White Castle Quadrangle, No. L–32; Napoleonville Quadrangle, No. L–33; Morgan City Quadrangle, No. L–34; Lake Decade Quadrangle, No. L–35; Oyster Bayou Quadrangle, No. L–36.

U.S. Geological Survey: Thibodeaux Sheet, Gibson Sheet, Bayou de Large Sheet, Lac des Allemands Sheet, Houma Sheet, Dulac Sheet.

STRONG RELIEF BEFORE BLOCK FAULTING IN THE VICINITY OF THE WASATCH MOUNTAINS, UTAH

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ABSTRACT

The theory is advanced that a submature to mature surface with relief of, at least, 3,000 feet existed in the vicinity of the Wasatch Mountains before the Basin and Range faulting began. The throw of the Wasatch fault in the Nebo section has been found to range from 5,000 to 6,000 feet. With present relief of 7,000 feet it is evident that the entire height of the range cannot be due to faulting. The Wasatch front in many sections displays typical fault-scarp topography from the piedmont to a height of approximately 3,000 feet. It then breaks suddenly into a submature surface of much lower gradient. Shoulders and large hanging valleys are striking remnants of this topography and are thought to indicate the amount of relief before the initiation of faulting. The drainage is interpreted as antecedent, because of the remnants of former mature valleys into which the deep, cross canyons are incised. The alluvium-flooded back valleys of the Wasatch antedate the faulting and are thought to have been filled with alluvium, because of lowering of gradient resulting from eastward tilting of the fault block. This tilting is calculated to range from 30 to 40 which is similar to Gilluly's figure for the Oquirrh Mountains.

INTRODUCTION

The idea is generally entertained as a result of the work of W. M. Davis and G. K. Gilbert that erosion had reduced the surface in the region of the Wasatch Mountains to a peneplain before the Basin and Range faulting began, and that the fault blocks now protruding above the alluvium of the valley fill owe their entire relief to faulting and their physiographic form to erosion on a fault scarp or on the back of a tilted fault block. In recent years, however, evidence has been accumulating which points to an interpretation somewhat at variance with the foregoing; namely, that a surface in the submature stage of the cycle of erosion was existent at the inception of the block faulting.

The older conception has become prevalent, possibly because of Davis' use of flat-topped blocks in discussing erosion on a fault scarp.[1] Since he measured the throw of the Wasatch fault by reconstructing the top of the upthrown block as a level surface, how-

[1] W. M. Davis, "Mountain Ranges of the Great Basin," *Harvard Coll. Mus. Comp. Zoöl. Bull. 42* (1903), pp. 129–74.

ever, he apparently pictured a peneplained surface broken by the Basin and Range faulting.

Gilbert[2] measured the throw of the Wasatch fault in the same way, from which it appears that he also visualized a fairly flat surface previous to the faulting. On the other hand, he speaks in the same article of rivers which transect the Wasatch Mountains as antecedent, thereby conveying the impression that some considerable relief existed before the faulting began.

Hintze[3] has accepted Davis' views and discusses the dissection and drainage of the central Wasatch block upon the basis of peneplanation with consequent drainage after block faulting.

The newer conception of strong relief previous to the faulting has been suggested by several geologists in recent years, but Beeson[4] was the first to publish his ideas. He described a mature erosion surface around the eastern slopes of Mount Timpanogos which he called the "old plateau." He believed this plateau to have been present before the Basin and Range faulting began, and, therefore, that Mount Timpanogos stood in striking relief to the surrounding country in that prefaulting time.

Gilluly[5] has recently described a mature topography in the Oquirrh Range which is older than the present expression of block faulting, and has noted also a supposedly prefaulting, early-mature surface on the east slopes of the Wasatch Mountains.

R. E. Marsell,[6] of the University of Utah, has recognized remnants of an old erosion surface at various points in the eastern part of the Basin and Range Province and has suggested to the writer the possibility of considerable prefaulting relief.

Although the idea of prefaulting relief is not new, it is not well known by geologists in general. The writer believes, therefore, that

[2] G. K. Gilbert, "Studies of Basin-Range Structure," *U.S. Geol. Surv. Prof. Paper 153* (1928), pp. 50–51.

[3] F. F. Hintze, "A Contribution to the Geology of the Wasatch Mountains, Utah," *N.Y. Acad. Sci. Annals*, Vol. XXIII (1913), pp. 88–91.

[4] J. J. Beeson, "Mining Districts and Their Relation to Structural Geology," *Trans. Amer. Inst. Min. and Met. Eng.* (preprint 1500–1), September, 1925.

[5] James Gilluly, "Basin Range Faulting Along the Oquirrh Range, Utah," *Geol. Soc. Amer. Bull.*, Vol. XXXIX, No. 4 (1928), pp. 1123–24.

[6] Oral communication.

a further treatment of the subject, setting forth the major lines of evidence upon which the theory is based, is warranted. He is also of the opinion that a more acceptable solution of some of the other problems of the late geologic history of the region in question can be worked out when strong prefaulting relief is considered.

EVIDENCE FROM STRUCTURE

During recent mapping of the southern Wasatch Mountains the writer found portions of downthrown blocks along the Wasatch fault unburied by alluvium, and a rough measurement of the fault displacement possible.[7] The throw on the north side of the mouth of Santaquin Canyon is about 5,500 feet, and on the south side approximately 6,000 feet. At the mouth of Wash Canyon the throw is about 5,000 feet, but is thought to be somewhat greater farther south. A fairly safe maximum figure for the fault displacement is, therefore, most probably 6,000 feet.

But the difference in elevation between the valley fill west of the fault and the top of Mount Nebo is 7,000 feet and the alluvium in places is more than 2,000 feet deep. It is, therefore, evident that the total relief cannot be a result of faulting. In order to account for the present relief in relation to the fault displacement, an initial relief of 2,000 or 3,000 feet must be postulated.

No actual measurements of the total throw of the Wasatch fault have been made farther north than Payson Canyon.[8] This is because the downthrown block is not exposed, as it is at various points in the southern Wasatch Mountains. Nevertheless, there are few points of greater relief than those in the Nebo region and it is, therefore, not probable that the throw of the fault at Provo Peak, Mount Timpanogos, Twin Peaks, or Ogden Peak is of greater magnitude. It is thus indicated that considerable relief existed along the entire Wasatch before faulting began. This argument, however, rests on the interpretation and measurement of the throw of the fault in the southern Wasatch Mountains.

[7] A. J. Eardley, "Stratigraphy, Structure, and Physiography of the Southern Wasatch Mountains, Utah." Presented to Princeton University in candidacy for the Ph.D., April, 1930. Not yet published.

[8] R. E. Marsell has not yet announced the results of his survey of the Traverse Range and may have measured the fault displacement there.

EVIDENCE FROM TOPOGRAPHY

SHOULDERS AND HANGING VALLEYS

The topographic features herein described as "hanging valleys" occur at a number of places on the Wasatch front and are from $\frac{1}{2}$ mile to over 1 mile in width and terminate in the fault scarp about 3,000 feet above the piedmont. They are major relief features and probably represent tributary valleys which formerly debouched into a trunk stream valley now submerged under the alluvium of Salt Lake, Utah, and Juab valleys (see Fig. 1). Conspicuous shoulders on the west slopes of Mount Nebo (Fig. 2) occur approximately 3,000 feet above the valley floor. The writer has attempted an explanation of their origin, the essentials of which are represented in the diagrams of Figures 3*A*, 3*B*, and 3*C*. The initial topography conceived is one in which a submature valley has been obliquely truncated by a fault and is left hanging on the fault scarp (Fig. 3*A*). Erosion upon the scarp immediately ensues, the shallow gullies of the submature valley walls developing into canyons (Fig. 3*B*). The downthrown block becomes covered by alluvium which extends in rather steep alluvial cones up the fault scarp to the mouth of the gullies.

The throw of the fault is represented as having been increased as the erosion progressed. The progressive character of the movement is attested by the following evidence; first, recent displacement on the old fault plane; second, noticeable lack of entrenchment in the apexes of the alluvial fans whose gradients are being maintained or increased by the progressive lowering of the downthrown block; and, third, the evidence furnished by all historical faulting.

A further stage of erosion is depicted in Figure 3*C*. This shows the nature of the surface at the present time and should be compared with the photograph, Figure 2. One of the shoulders has collapsed and a great pile of landslide material has formed on the piedmont at the base of the fault scarp. The actual fault scarp, as depicted above, has been modified by erosion in harmony with Davis'[11] deductions. The slopes of Mount Nebo above the hanging valley do not display similar forms and hence have been affected by the faulting only as far as rejuvenation of the draining has occurred.

An alternative hypothesis for the shoulder and trough on the west

face of Mount Nebo is that they marked a fault branching off from the main Wasatch fault. The region has been rather closely studied,

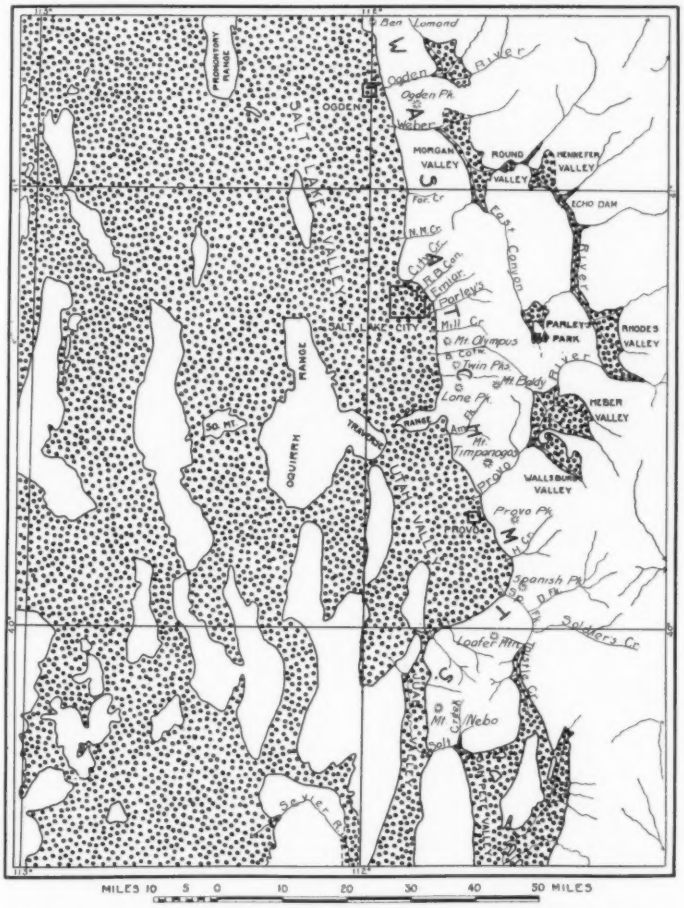

FIG. 1.—Map of eastern portion of the Basin and Range Province in the vicinity of the Wasatch Mountains showing distribution of alluvium and bed rock. Alluvium is stippled area and bed rock is white area.

however, and no fault is known along the trend of the trough. The trough is a strike valley in highly inclined and vertical sedimentary

beds. A shale member about 200 feet thick forms the bottom of the trough, a fact as favorable to the idea of a prefaulting strike valley as to that of valley development only in postfaulting time.

The summit of Mount Nebo is 3,300 feet above the hanging valley. A relief of about this magnitude, therefore, is believed to have existed in the vicinity of the southern Wasatch Mountains[9] before the inception of the Basin and Range faulting.

FIG. 2.—View of west slopes of Mt. Nebo. Compare with figure 3*A*

The best example of hanging valleys in the Wasatch Mountains occurs in the Provo Peak section. Figure 4*A* is a line drawing traced from a photograph looking north-northeast at two of these hanging valleys, one of which heads along the western slope of Provo Peak and the other along the southern slope. This is the portion of the Wasatch front made classic by W. M. Davis.[1] It will be observed that the faceted spurs hold true to Davis' deductions of erosion on a fault scarp, but only for the lower half of the mountain front. At an elevation of about 3,000 feet above the piedmont the typical fault

[9] Since the height of the shoulders or hanging valley above the piedmont on the west slope of Nebo is about 3,000 feet and the throw of the fault is between 5,000 feet and 6,000 feet, the depth of alluvium in the downthrown valley block at the fault must be somewhat less than 3,000 feet, the exact depth depending on variations in displacement and also irregularities in relief of the submerged block.

scarp topography breaks, abruptly in places, into a submature surface with gradients decidedly lower than those of the fault scarp.

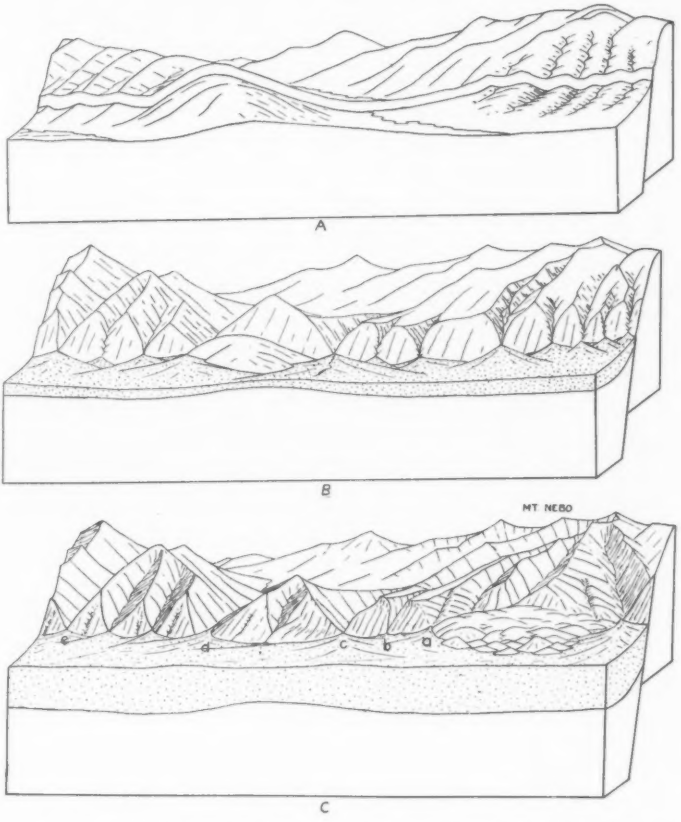

FIG. 3.—*A*. Writer's conception of topography at western base of Mt. Nebo at inception of block faulting.

B. With continued faulting and fresh incision of the fault scarp.

C. With total displacement and erosion to present time.

The letter *e* denotes the position of North Canyon; *d*, Pole Canyon; *c*, Bear Canyon; *b*, Dry Canyon; and *a*, Couch Canyon.

A well-formed valley at *A*, Figure 4*A*, is truncated, and at this point the upper extent of the true fault scarp is defined. At points on the range front other than at the mouth of valley *A*, the upper extent of the fault scarp is somewhat higher because of the irregularity of the

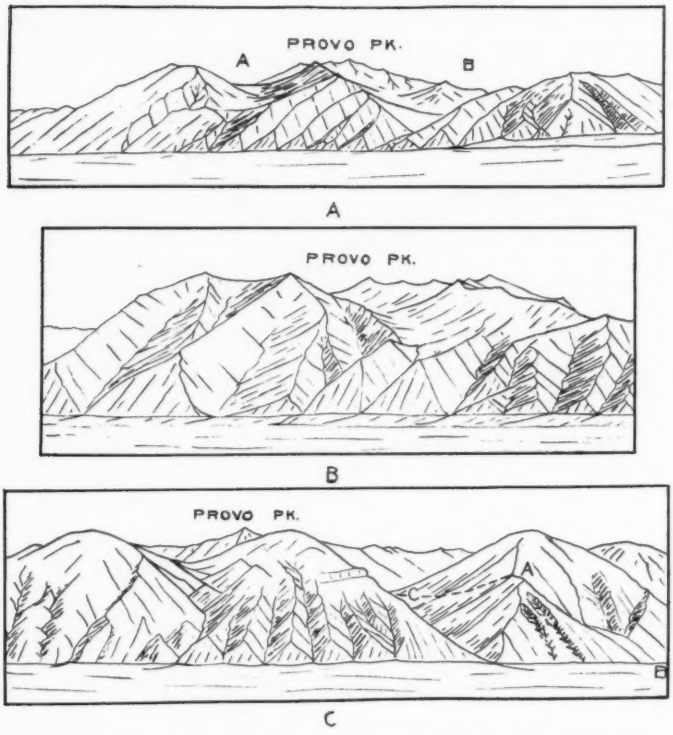

FIG. 4.—*A*. Line drawing traced from photograph of hanging valleys on southwest side of Provo Peak.

B. Another view of hanging valley *A* of Figure 4*A*.

C. Line drawing of Figure 3*A* showing fault and its small effect on topography.

surface cut by the fault, the valley at *A* being the point of lowest relief, locally, of this prefaulting surface. Another valley at *B* is less well defined, but is distinctly different in appearance and of lower

gradient than the topography below on the fault scarp. Figure 4*B* is a closer view of valley *A*.

Provo Peak is almost 3 miles back of the "break" in the topography on the mountain front. To account for erosion having reduced a fault scarp which once extended as high as Provo Peak in such a manner as to leave points of highest relief in a submature topography far back of the main "front" and subtended above a true fault scarp topography, would be difficult if the total relief is thought to be due to faulting. The idea of two episodes of faulting with a long interim of quiet may be looked to as a possible answer. This idea will be discussed after the remaining evidence is presented.

Additional evidence for the theory of prefaulting relief is obtained if Provo Peak is examined from the west (at a point several miles northwest of the viewpoint of figure 4*B*). Three great triangular faces (Figs. 4*C* and 5*A*) have been sculptured out of the fault scarp by regularly spaced canyons. These faces extend up to elevations of about 3,500–4,000 feet above the piedmont, but are, however, considerably lower than Provo Peak, the snow-capped pyramidal point in the background. The truncated valley *A* of Figures 4*A* and 4*B* lies between these frontal shoulders and Provo Peak. Its headwaters have been tapped and stolen by the canyon denoted by *C* in Figure 4*C*.

Unlike the truncated and hanging valley on the west face of Mount Nebo, the hanging valley on the west of Provo Peak is implicated with a fault which might be summoned to explain its existence. The Wasatch fault, in the light of the above suggestion, would consist of two faults in step arrangement, thus producing a terrace or shoulder of intermediate relief between the main upthrown and main downthrown blocks. The great triangular faces would be the result of erosion in the lower escarpment, and the hanging valley (*A*) would have originated on the back-slope of the intermediate fault block. Dr. Schneider, of the University of Utah, who pointed out the fault to the writer in the field, finds that it runs into the main Wasatch fault without continuing parallel with it throughout the entire length of the hanging valley. Figure 4*C* shows the position of the fault. It leaves the hanging valley at *C*, continues to a small shoulder *A*, and then runs into the main fault at *B*. Because of the

A

B

FIG. 5.—*A*. A view of Wasatch front east of Provo showing pyramidal Provo Peak in the background high above the three great triangular facets of the fault scarp.

B. View of the eastern drainage of Provo Peak looking south-southwest from north side of Provo Canyon. Compare with Figure 8*A*.

disrespect of the major topographic features for this fault, an interpretation which holds their origin to be entirely the result of it cannot be logically entertained. The writer believes, however, that this fault increases in throw northward and that in the Timpanogos section it has had a marked effect upon the topography.

The topographic features of the Provo Peak section of the Wasatch front are similar to those of the Nebo section and are interpreted as recording the same physiographic story; namely, that there existed a relief of 3,000 feet or more before the Basin and Range faulting began.

Continuing the examination northward, the section north and south of Provo Canyon is found to contain a very conspicuous frontal ridge which immediately arrests attention. This feature was first described by Emmons,[10] but later both illustrated and described by Gilbert.[11] The evidence of prefaulting relief which it furnishes is incompletely understood and, therefore, of such nature as not to warrant repetition of description and illustration. Emmons[12] recognizes "faultings" in the gorge section of Provo Canyon and American Fork Canyon, while Gilbert[13] points out three "internal faults" back of the main frontal fault and parallel with it. The writer believes that the westward displacement has been accomplished by two major step faults and that the fault-bounded step block itself may be broken by other minor parallel faults. Suffice it to say that those who have mentioned this frontal ridge in writing, and those with whom the writer has conversed, believe that the essential factor in its formation was faulting. The extent to which initial relief influenced the present surface expression here will not be known until detailed mapping has been done and the throw of the faults determined.

In the section of the Wasatch Mountains embracing Lone Peak, Twin Peaks, Mt. Olympus, and Perkes Peak (the next high peaked division of the Wasatch north of Mt. Timpanogos) a great granitic rock mass has wrought a material change in the aspect of the range.

[10] Arnold Hague and S. F. Emmons, "Descriptive Geology," *U.S. Geol. Expl. 40th Par. Rept.*, Vol. II (1877), p. 345.

[11] *Op. cit.*, pp. 64–66; also Fig. 44 and Pls. 21*a* and 31*a*.

[12] *Op. cit.* [13] *Ibid.*, pp. 65–66.

Here it is much wider, with an assembly of high peaks instead of a single ridge. It is the junction of the Uinta Mountains with the Wasatch and is marked by several large igneous intrusions along a projection of the Uinta Mountains axis. Figure 6*A* represents a front view of this section looking southeast.

The idea is advanced that the granite intrusion known as the "Cottonwood Stock" and the hard Cambrian and pre-Cambrian quartzites to the north constituted a mountainous terrane standing

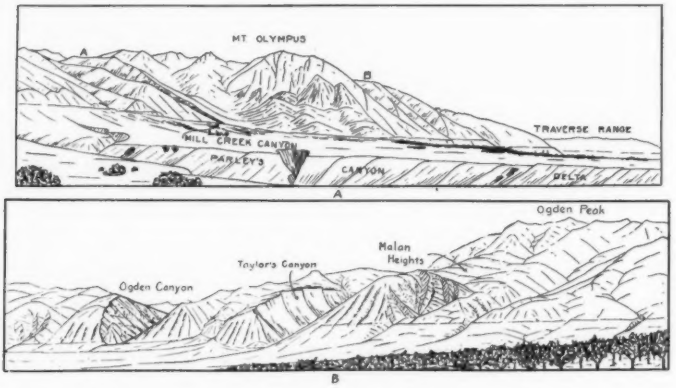

FIG. 6.—*A*. Line drawing traced from photographs of Mt. Olympus in the central Wasatch looking northeast showing meager remnants of valley floor before faulting, *A* and *B*.

B. Line drawing sketched from photographs of Wasatch front east of Ogden showing prefaulting erosion surface.

3,000 feet above lower country to the north and west before the inception of block faulting. The Uintas probably carried the relief eastward as did the high single ridge southward. The Wasatch fault, which broke this topography, swings westward and skirts closely the periphery of the Cottonwood Stock, thereby accentuating the relief of the mountainous terrane, but leaving either no shoulders or very inconspicuous remnants.

This picture of prefaulting topography in the Cottonwood Stock area is built upon the following evidence. First, the Wasatch Mountains from Mill Creek Canyon north through City Creek

Canyon and the southern portion of the Archean complex north of Salt Lake City do not attain the elevations of the high peaks previously mentioned and, therefore, represent the area of lower relief just postulated. It is difficult to account for an entire section of the range 2,000–3,000 feet below the peaks of an adjoining section and, in particular, for such level-topped ridges, as at *A*, by erosion on a fault scarp. Second, the shoulders at *B* indicate that the western margin of the granite stock marked the western margin of the area of higher relief. Their narrowness indicates that the Wasatch fault closely followed this boundary in preference to transecting the stock itself. Third, the explanation of the relief of the Cottonwood Stock is in keeping with the interpretation of the physiography of the range farther south.

As a last example of prefaulting relief on the Wasatch front, the Odgen Peak section will be considered. Figure 6*B* is a line drawing of a photograph of this section showing the broad, smoothly rounded shoulder (Malan Heights) with the high mass of Ogden Peak in the background. The writer interprets this topography in the same manner as in former cases; namely, that faulting has added to the relief of Ogden Peak only an amount equal to the elevation of Malan Heights above the valley floor. This conclusion is based on (1) the uniformly lower elevations north of Ogden Peak; and (2) the fresh incision of Ogden and Taylor canyons in this lower lying, mountainous tract.

To account for the first item by erosion on a fault scarp would be somewhat difficult unless special conditions are postulated. One of these would be an interim of quiet during the faulting whereby the less resistant rocks become eroded down to the elevation of Malan Heights and then are brought into relief again by a second episode of faulting. The writer does not believe the shoulders below Ogden Peak or elsewhere in the Wasatch to be the result of episodes of faulting, because the land forms above the shoulders bear no resemblance to fault-scarp topography, even in the fan-bayed stage of the cycle of erosion. Another condition that might be postulated for the difference in relief between Ogden Peak and the lower mountainous country to the north would be the presence of a normal east-west fault along the boundary of the two areas. Singularly, a major

fault does exist approximately at this boundary. It has been described and named the Huntsville fault by Blackwelder.[14] The downthrown side is apparently to the south although considerable horizontal displacement is thought to have occurred. This is just the reverse of relations necessary to account for the present area of lower relief to the north. In addition, Blackwelder says, "The transverse faults, including the Huntsville fracture, have no topographic expression, except that which is due to the relative resistance of

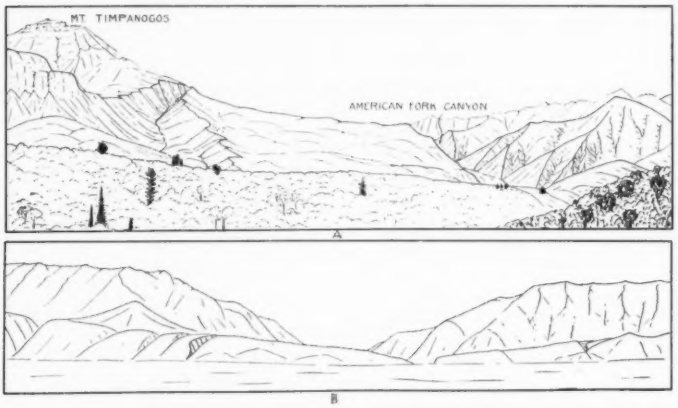

FIG. 7.—*A*. Line drawing traced from photographs of Mt. Timpanogos and headwaters of American Fork Canyon looking slightly north of west, showing old erosion surface.

B. Sketch of mouth of Provo Canyon showing incision of gorge into old, mature valley bottom.

the rocks on one side or the other. The scarps have been wholly removed by erosion" (p. 541). Transverse faulting is therefore denied as a possible cause for the difference in relief of the two areas in question.

In many places on the Wasatch fault scarp shale forms benches and quartzite forms cliffs, but in only a very few instances could the shoulders thus formed be mistaken for those of much greater magnitude from which evidence has been drawn for prefaulting relief.

[14] Eliot Blackwelder, "New Light on the Geology of the Wasatch Mountains, Utah," *Bull. Geol. Soc. Amer.*, Vol. XXI (1910), p. 540–41.

CROSS CANYONS

Most of the streams of the Wasatch Mountains head in the area to the east, flow westward in deep, narrowly incised canyons through the range, and empty into the alluvium-filled valleys to the west. The gorges of such streams through the mountains have been called "cross canyons" by Gilbert[15] and have been discussed to some extent by Howell[16], Gilbert [17], Hintze,[18] Anderson,[19] Lee,[20] and Beeson.[21]

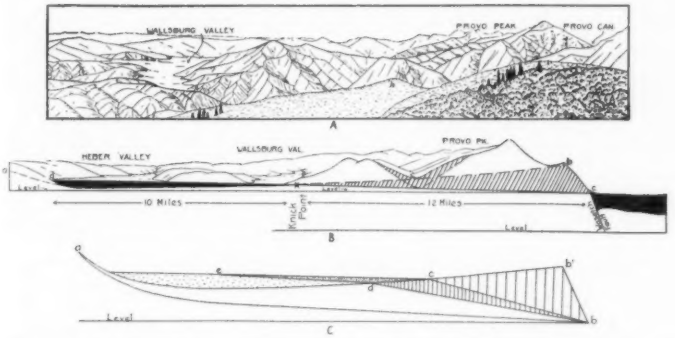

FIG. 8.—*A*. Line drawing traced from photographs of the country east of Provo Peak. View looking south.

B. Longitudinal profile of Provo Canyon showing antecedent erosion (lined) and deposition (black) upon tilting of the fault block.

C. A study of terraces formed about the migrating knick point of Figure 8*B*.

A general idea of the topography of the area drained by these streams may be obtained from Figures 8*A* and 8*B*. Provo River heads in the Uinta Mountains, flows southwestward through the wide-bottomed, mature Heber Valley and then through the great

[15] *Op. cit.*, p. 55.

[16] E. E. Howell, "Report on the Geology of Portions of Utah, Nevada, Arizona, and New Mexico Examined in the Years 1872 and 1873," *U.S. Geog. and Geol. Surv. W. 100th Mer.*, Vol. III, pp. 227–301. (See especially pp. 251–52.)

[17] *Op. cit.*, p. 55. [18] *Op. cit.*

[19] G. E. Anderson, "Stream Piracy of the Provo and Weber Rivers, Utah," *Amer. Jour. Sci.*, 4th ser., Vol. XL (1915), pp. 314–16.

[20] W. T. Lee, "Guidebook of the Western United States, Part B, The Overland Route," *U.S. Geol. Surv. Bull. 612* (1915), pp. 97 and 102 nn.

[21] *Op. cit.*, p. 29 and Fig. 8.

gorge of Provo Canyon. A sister stream to the Provo (see Fig. 1) is the Weber River, which likewise heads in the Uintas, flows west and north through a series of open valleys, and then westward through the Wasatch Mountains. To the north of Weber River is another cross canyon, that of Ogden River, whose tributaries extend at least 10 miles east of the high summits of the range. To the south of Provo Canyon, is Spanish Fork Canyon which also drains a considerable area to the east. In the central part of the range are numerous smaller streams, most of which head in divides east of the highest crests of the Wasatch though they drain much smaller areas than do the streams of the main cross canyons.

With this reconnaissance picture of the topography in mind, the various theories regarding the origin of the cross canyons will be outlined and then criticized in light of the theory of prefaulting relief.

There is the idea of consequent drainage on the tilted, supposedly flat-topped fault block of the Wasatch Mountains. The dissection of such a block would have been initiated by the consequent streams which flowed down the unequal slopes to the east and to the west. The unequal declivity of the two sets of streams would in time have caused a migration of their divide backward from the fault scarp or toward the east. This eastward migration of the drainage divide is postulated to have proceeded so far, in the case of Weber River and Provo River, as to have captured all the drainage of the eastern slopes of the Wasatch and much of that of the western end of the Uintas. Such is the view advocated by Hintze (pp. 89–91) and Anderson (p. 314).

Another theory of the origin of these cross canyons is based on the concept of antecedent drainage. A few large, east-west, prefaulting streams are assumed, their valleys deep enough to control the drainage of the region after the block faulting across their courses. Further amplification of this general assumption has followed two divergent lines of thought. In one, the rivers are imagined to have flowed from west to east and, because of the faulting, to have been reversed in direction of flow. The notches which these streams made in a postulated prefaulting Wasatch ridge controlled the courses of fu-

ture, westward-flowing streams. Howell[22] has briefly suggested this explanation. In the other idea the rivers are conceived to have flowed from east to west and to have held their courses during the development of the range. Lee[23] is the proponent of this view. He says, "Ogden River was flowing west along its present course before the Wasatch Mountains came into existence. The raising of the mountains went on slowly for ages, so slowly that the river kept its place by cutting down its ever-rising bed, carving a deep and narrow canyon straight through the block of the earth's crust as it rose."

Gilbert[24] agrees with Lee except that he is not sure whether the present cross drainage of the streams antedated the entire growth of the range or was consequent on some stage of its growth.

These divergent views raise the following problems:

1. Amount of relief before faulting.
2. Direction of cross drainage, if any, before faulting.
3. Nature of faulting.
   a) A continuous series of small displacements, or episodes of faulting with quiet interludes.
   b) Simple settling of valley block, or tilting of mountain block, or both combined.
4. A correct understanding of the development of a drainage system upon the topography which the solution of the above three problems will determine.

The first problem has been treated to considerable extent in the preceding section on shoulders and hanging valleys in which the conclusion was reached that a preblock-faulting relief of at least 3,000 feet may be postulated. In addition, the cross canyons themselves lend conviction to this view. The cross canyons are simply great gorges freshly incised into an older surface, remnants of which are still distinctly visible. It is the strong relief of this older surface that furnishes the additional testimony.

An excellent example is American Fork Canyon, the stream of which has cut deeply into a wide mature valley of earlier age. Figure 9 is a view of this feature looking somewhat north of west

[22] *Op. cit.*, pp. 251–52.

[23] *Op. cit.* [24] *Op. cit.*, p. 64.

FIG. 9.—Panorama of eastern slopes of Mt. Timpanogos and headward drainage of American Fork Canyon. Compare with Figure 7A

down the canyon. Figure 8 of J. J. Beeson's publication[25] is another view equally good. Not only does a distant view show the old valley form, but a close inspection of the floor, now the shoulder area above the canyon, reveals it to be poorly drained with small bodies of standing water and, in places, stream gravels. The story unfolded is simply one of rejuvenation caused by the block faulting. The old valley floor terminates in the fault scarp at the same general elevation above the piedmont at which the hanging valleys do. Its age is ascribed, therefore, to preblock-faulting time for the same reason that the age of the smaller hanging valleys were; namely, the inability to deduce such forms by erosion on a fault scarp, even though the displacement occurred in episodes.

The cross canyon of Provo River is another example of rejuvenation on a grand scale. Because of the large amount of water passing through this

[25] *Op. cit.*

gorge, erosion has been more extensive, and, hence, the remnants of the old valley of Provo River are less distinct than are those of American Fork. However, they are still prominent and easily deciphered. Figure 7*B* shows these features.

Pre-eminent in clearness among the antecedent canyons of the Wasatch is Ogden Canyon whose gorge is sharply and deeply cut in the older, subdued terrane (see Fig. 6*B*). Weber Canyon, like Provo Canyon, is marked by smaller and less distinct remnants of the old valley floor because of a larger stream and greater erosion.

In several cases of cross canyons, especially the smaller ones, the evidence of incision is very vague or entirely wanting. This is to be expected because the tributaries normally display more youthful characteristics than do their master streams and, therefore, superimposition of a youthful and very vigorous stage of erosion upon these barely mature, tributary stream valleys would not leave much to show for their former existence. This is especially true of the canyons of the central Wasatch or Cottonwood Stock area.

The second problem which concerns the direction of cross drainage, is thought to find partial solution in the following observation. Had the cross streams flowed eastward before faulting began there would have been convergence to the east into one or more main streams which would have drained off either to the southeast into the Colorado Plateau or northward, possibly into the Bear River Valley country. There are no old outlets in these directions as far as the writer could determine either from field study or from the topographic maps of the region. The pass from Heber Valley to Strawberry Valley, which is in the Plateau drainage, is 8,000 feet high. The present floor of Heber Valley is 2,500 feet lower. With good initial relief before faulting it is not likely that the drainage flowed out through this possible pass. Similar elevations exist to the north. In fact, it would be difficult to find any possible exit of the drainage in that direction. The character of the present drainage area in its headward reaches suggests that of normal stream erosion, without such violent disturbances as reversal of drainage and its associated changes. This problem, together with the third and fourth ones, will be discussed under the subject of "back valleys."

BACK VALLEYS

A number of flat-bottomed, mature valleys containing from 1 to 30 square miles of flood plain area each are the outstanding topographic features of the eastern drainage of the Wasatch Mountains. The significant fact about these large, wide valleys, which Gilbert[26] calls "back valleys," is that they are flooded with alluvium. Their floor is quite flat and usually abuts sharply against the valley walls. In many places alluvial fans have contributed to the aggradational work of the main valley stream. It is evident that these valleys were once deeper than at present and that some kind of crustal disturbance has caused the deposition. The writer believes that they were eroded before block faulting began, and then, by eastward tilting of the fault block, the gradient of the streams was decreased and deposition occurred bringing about the present alluvium-flooded condition. The valleys drained by Weber and Ogden rivers present a very complex, and as yet not well-understood, problem in which graben faulting may have been a contributing factor.[27] Besides the possible complication by faulting of Basin and Range age there is an additional chapter written into the late history of Ogden and Morgan valleys; namely, the embayment therein of Lake Bonneville waters through the cross canyons. In these flooded valleys were deposited large amounts of deltaic material which greatly masks any previous depositional or erosional records of their earlier history.

The writer's argument is developed for Heber Valley and its tributaries, because in this locality neither faulting nor deposition in Lake Bonneville have interrupted the erosional history. Figure 8*A* is a line drawing traced from photographs of a portion of the valley of Provo River looking south. The gorge section and high peaks are seen at the right and the silted-up Wallsburg Valley at the left. Heber Valley is farther to the left, but not within the border of the figure. From the mouth of Provo Canyon upstream to a point at about the debouchure of Wallsburg Valley, a distance of 12 miles, the river flows on bed rock (small amounts of transient débris clog the channel) with a gradient of approximately 70 feet per mile. From this point upstream throughout the whole length of Heber Valley

[26] *Op. cit.*, p. 55. [27] *Ibid.*, pp. 65–66.

the river flows on valley fill with a gradient of only 20 feet per mile. The point at which alluvium ends and bed rock begins will be called the "knick point." This is marked *X* in Figure 8*B*. The longitudinal profile of the old valley floor under the valley fill may be extended westward beyond the knick point by tracing the shoulders of the freshly incised gorge which gradually increase in height until the fault scarp is reached. The writer interprets these features to record the following history. A previously existing river system heading in the Uintas and flowing westward through a mature valley across the line of faulting was disturbed by the eastward tilting of the fault block. The displacement along the fault occurred in small movements so gradual that the downward cutting power of the river was sufficient to maintain a course through the upturned block. The gorge section, however, constituted a new river profile independent of the profile above. Each new fault displacement only increased its gradient and added to its erosive power, thereby accelerating its speed of headward extension. On the other hand, the reverse was happening to the portion of the stream valley above the gorge whose base of deposition was the knick point. Its gradient was decreased with each new displacement with consequent deposition. A sufficient gradient was maintained to carry the waters of Provo River to the knick point by this deposition. As a result, at the present time Provo Valley and its larger tributary valleys above the knick point are silted up almost as if flooded with water. Gullies in the valley walls have built many alluvial fans upon the deposits of the river.

The deep incision of the gorge rejuvenated all tributary streams leading into it. Their valleys were deepened as shown in Figure 8*B*, but, because of the already youthful aspect of the prefaulting topography of the ridge section, not much expression remains. It is thought that shoulders are discernible in the valley to the east of Provo Peak, Figure 8*A* and 5*B*, and that they signify this rejuvenation. On the east flanks of Mount Nebo the shoulders are much more distinct.

The knick point is not stationary, but is moving headward even at the present time. Figure 8*C* shows the configuration of terraces formed as a result of this migration. The line *ab* represents Provo River profile before tilting of the fault bock, and *ab'*, after tilting.

Had no erosion of the gorge section occurred, the valley fill would have extended to the fault scarp, b', and would have become thickest in some back part of the valley at about point e as tilting continued. The gorge section, however, in the early stages must have been cut rapidly headward because of its very steep gradient, thereby lowering and driving eastward the base or knick point, upstream from which deposition occurred. It is thought that only small amounts of sediments were deposited downstream from point c because of the rapidity of gorge erosion back to this location. However, further lowering of the gradient became a much slower process, thus creating a more nearly permanent baselevel. With the flood plain of the river built up to c and with further migration of the knick point to d, a terrace on either side would be formed which would gradually die out upstream toward e and become wedged out downstream and lost against the canyon walls. The actual presence of such terraces at the knick point in Provo Canyon supports these deductions. From the present knick point, d, upstream, a small, but well-defined terrace may be observed on the northwest side of Heber Valley, which decreases in height upstream and finally disappears in about 1 or $1\frac{1}{2}$ miles. Between d and c the canyon is so narrow that wide terraces are not to be found; and in places all terrace material has been removed. The most striking remnants of a former valley fill are the truncated alluvial fans which embay the gullies and which were once built out to and upon the river flood plain, now eroded away. They occur on both sides of the valley, become increasingly higher above the canyon bottom downstream, and line up approximately with the shoulders of the gorge section. They are interpreted as confirmatory to the theory of antecedent drainage over a tilting fault block and hence as additional evidence for preblock-faulting relief.

The foregoing study has attempted to answer the remaining problems which the divergent views of other writers concerning the prefaulting drainage had raised; namely, the existence of cross drainage and the direction of flow, the nature of faulting, and the development of a drainage system on a tilting fault block. There remains one topic of interest, the problem of the degree of tilting. Because of the eastward dip of the thrust planes of the Laramide Revolution in the Wasatch Mountains and because of the postulated west-to-

east movement of the overriding fault blocks, it might be thought that the subsequent block faulting, in which the Wasatch block was tilted eastward, was the cause of the eastward dipping thrust planes, which in Ogden Canyon are measured at about 30° E. This figure seems excessive. The physiography of the region yields a more tangible basis upon which to figure the amount of tilting. From Figure 8*C* it is quite evident that if the elevation of point *b* were known, i.e., the gradient of the stream profile before faulting commenced, the amount of tilting could be readily determined. The elevation of this point can be approximated only, so far as the writer knows, by assuming a gradient somewhat less than the present, because of the more mature aspect of the older valley, and by extending it to the fault scarp from some point well back in Heber Valley. This yields a difference in elevation between *b* and *b'* of about 3,500 feet. If we take this as the vertical side of a right angle triangle and about 20 miles as the base, an angle of tilting of 2° is obtained. This calculation is based on the assumption that the whole block moved rigidly about a definite hinge line 20 miles back from the fault. This is probably not exactly true. The writer believes that the position of the hinge line is indefinite, and that the Wasatch block suffered a warping deformation with tilting progressively greater toward the fault scarp. If the base of the triangle is foreshortened to 12 miles, the angle of tilting is found to be 3° 7′ which is perhaps closer to the actual amount of tilting along Provo Canyon. An angle as high as 30° is certainly not in accord with this evidence. The low degree of tilting is verified by the position of a limestone formation of Eocene age flanking the east slopes of Mt. Nebo in the southern Wasatch, older than the block faulting, but still little disturbed by the eastward rotation.[28]

If the Wasatch block was tilted up at the fault plane only 3,500 feet, there must have been an actual downward movement of the valley block west of this, for the Wasatch fault certainly has a greater throw than 3,500 feet. Neither block stood still in relation to sea level, one rose and the other sank, probably both tilting eastward.

[28] Gilluly calculates an eastward tilting of about 4° in the Oquirrh Mountains which corresponds rather closely to the writer's figure for the Wasatch Mountains (*op. cit.*, pp. 1121–22).

BED ROCK ISLANDS

As a last argument in favor of prefaulting relief, certain features which the writer refers to as "bed rock islands" will be briefly discussed. They project through the alluvium much like islands along a subsiding coast of strong relief. They vary in size from 100 feet in diameter to small ranges and are found in many localities in the region immediately west and south of the Wasatch Mountains. They are well shown in Great Salt Lake and Great Salt Lake Desert and in the north end of San Pete Valley. South Mountain, in Tooele Valley, and Promotory Range are other examples. It is difficult to account for their irregular ground plan outlines by faulting, especially where no discernible fault-scarp topography is found. The geologic map of the state of Utah shows no faults bounding many of these alluvium surrounded islands. An alternative explanation to block faulting is strong preblock-faulting relief or a combination of the two. This latter idea readily lends itself to an interpretation of the irregular, bed rock islands as peaks of a mature region, here and there accentuated by faulting, protruding above the valley fill. The difficulties encountered in explaining them solely by faulting strengthens the value of this alternative view.

The abrupt end terminations and irregularities in outline of some of the larger, definitely block-faulted ranges suggests also the possibility of prefaulting relief as partly responsible for their present configuration, although not much stress may be laid on this point until specific examples can be cited and detailed work has been carried out. The various spurs of the Wasatch and the irregular ranges southwest of Utah Valley may prove to be in this class.

ECONOMIC SIGNIFICANCE

The theory of strong preblock-faulting relief, if found to be correct, will have decided value in discovering and delimiting various ground water basins in the alluvium-filled valleys to the west of the Wasatch which, from meager well records, seem to exist. It is postulated that the points of strongest relief find expression at the surface by protruding through the alluvium, but that many smaller irregularities in the old valley floors will be reflected in the sediments covering them. There should, then, be definite channels of more

abundant circulation, especially at depth, and it may be possible with more work to map these channels approximately without the expense of prospecting by drilling.

If the sediments of the alluvial fill cover hills or ridges, the probability of reflected structures suitable for oil accumulation is indicated. Before this application of the theory becomes of commercial value, however, many other factors of the origin and accumulation of oil must be investigated.

CONCLUSION

The attempt has been made to show from the structure and topography of the Wasatch Mountains and vicinity that the theory of peneplanation before block faulting is probably not correct and that the present topography of the ranges appears to be better interpreted in the light of strong preblock-faulting relief. A difference in elevation of at least 3,000 feet from mountain crest to valley floor in the old prefaulting-erosion surface is believed to have existed.

ORIGIN OF THE CONCRETIONARY STRUCTURES OF THE MAGNESIAN LIMESTONE AT SUNDERLAND, ENGLAND

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ABSTRACT

The Magnesian limestone (Permian) near Sunderland on the east coast of England exhibits a series of remarkable concretionary structures. The forms of these structures are extremely varied and intergrown, producing what are probably the most remarkable patterns in sedimentary rocks found anywhere in the world. The exposures of the rocks containing them appear to be covered by a lovely lace drapery. Some forms are spherical, and others are aggregates of rods. The occurrence and origin of these structures are discussed.

INTRODUCTION

Probably few, if any, rock formations show such a remarkable group of concretionary structures as does the concretionary member of the Magnesian limestone in the vicinity of Sunderland, Durham County, England. This is the more remarkable as the formation is limited both in thickness (the member has a maximum thickness of about 250 feet) and areal extent. Some of the concretionary types, especially the columnar and rodlike aggregates, are not known to occur elsewhere in the world. A splendid exposure (about 20 feet high and 100 feet long) occurs in a face of an abandoned portion of the Fulwell Hill quarries at Sunderland. These concretionary structures resemble a lovely lace drapery, as will be seen by an examination of Figure I. During long exposure (it is estimated that this face has been exposed about 100 years), all loose material has been removed, leaving the concretionary structure exhibited in all its delicate tracery. This exposure should be permanently preserved as one of the most outstanding examples of nature's ability to build artistically in stone. The other dominant mode of occurrence of these structures is a globular form; and although this type is not uncommon elsewhere, its occurrence here is rather unusual.

These unique deposits have been the object of considerable discussion during the last 100 years. In this paper, only sufficient descriptive details of the structures are given to enable the reader to

understand the discussion of their origin. In fact, a word picture of the infinite variety and interrelationship of these forms, even in the

FIG. 1.—Concretionary structure in north face of an old portion of Fulwell Hill Quarries, Sunderland, England. Note the lace-like character of these concretionary structures and the relationship of the round and elliptical forms to the joints in the magnesian limestone. Scale: 1 inch equals 6 inches. Photographed for George Abbott by D. Johnson.

hands of a Ruskin, would inadequately present them to the reader. They must be seen to be fully appreciated. The figures accompanying this paper give a small concept of their beauty.

GENERAL GEOLOGY OF THE AREA

The Magnesian limestone of Durham County belongs to the Permian deposits of Northeastern England. The Permian sea, advancing from the east, deposited first a sandstone, then a thin shale bed, and then nearly 1,000 feet of dolomite and limestone. This series, which corresponds to the Zechstein of Germany, is gypsiferous at the top, and grades upward into the upper Red Beds, which consist of marls, cross-bedded sandstones, and thin dolomitic limestones, with beds of salt, anhydrite, and gypsum scattered through them.

The Magnesian series is divided into lower, middle, and upper members. The lower member is about 250 feet thick and consists chiefly of dolomite and dolomitic limestone.

The middle member is somewhat fossiliferous and contains a remarkable bryozoan reef that is from 1 to 2 miles wide, about 20 miles long, and 300 feet thick. This reef formed about 10 or 12 miles offshore and acted as a barrier; thus the rocks on each side are somewhat different. The middle member consists dominantly of dolomite, but contains some limestone besides that of the reef.

The upper member of the Magnesian series consists at the base of a "flexible" limestone (12 feet), then the concretionary member (250 feet), and at the top an oölitic dolomite (100 feet). The concretionary member occurs only on the east, or sea, side of the bryozoan reef. This member is called the "concretionary limestone," because it consists dominantly of limestone with occasional beds of dolomite. A surprising feature of this dolomite is that it is still soft and powdery (it is called "dolomite marl") even though it is of Permian age. It occurs in massive beds and is buff to yellow in color. The limestone is hard and crystalline, dominantly light gray, and occurs in beds ranging in thickness from 8 feet to a fraction of an inch. The limestone shows considerable cross bedding, the intersecting planes of which indicate variations in direction of the ocean currents.

The texture of the beds of the upper member is, of course, variable, that of the marls and some of the dense limestones being ex-

tremely fine and other phases medium grained. Some beds are porous, and others are dense. As the result of subsequent deformative movements, fracturing and fissuring are widespread. Faulting accompanied these movements, though few of the faults have a displacement of more than a few feet. There are many intersecting vertical, or nearly vertical, sets of joints, which are from 3 to 8 inches apart. Inclined joints are also numerous. Many of these joints and faults have been passages for ground water and are more or less completely filled with calcite. Some portions of the concretionary member are so badly shattered and displaced as to simulate a breccia; in fact, some geologists have called them "breccias." Stylolites occur in various parts of the formation. Most of them are mere stylolitic seams which do not exceed $\frac{1}{8}$ inch in thickness, though some are $\frac{1}{2}$ inch thick. The deformative movements have warped the area into broad, gentle folds.

ORIGIN OF THE CONCRETIONARY FORMATION

The origin of the concretionary formation involves the origin of dolomite (long a controversial question) and limestone. The writer believes that all dolomites and many limestones are chemical deposits. Permian formations furnish exceptionally strong evidence of this, especially the Permian dolomites associated with distinctively chemical sediments like gypsum and salt. The widespread, epicontinental Permian sea was an ideal place for the deposition of dolomite. The writer,[1] some years ago, mapped the paleographic distribution of dolomites, which showed that dolomite had been deposited dominantly in seas similar to the Permian. Such an interior sea favors the accumulation of magnesian salts and the development of the physico-chemical factors that are essential for the precipitation of dolomite (and gypsum and salt). Limestone, also, is readily precipitated in such a sea.

The writer, after years of study of this problem, believes that dolomite is deposited directly as the double salt, rather than that the $CaCO_3$ is deposited first and then changed to the double salt by the addition of $MgCO_3$. Much of the experimental work done has sought to show that subsequent replacement is the more probable method,

[1] W. A. Tarr, *Bull. Geol. Soc. Amer.*, Vol. XXX (1919), p. 114.

as chemists are unable to precipitate the double salt except at temperatures impossible in a sea. We must not forget, however, that Nature's adjustment of the physico-chemical factors in a multiple-phase system far transcends anything attained by man. The long list of chemically formed sediments attest to this fact.

During the formation of the Magnesian limestone, it is evident that there was usually sufficient $MgCO_3$ to combine with the $CaCO_3$ to form dolomite, as dolomites predominate throughout most of the formation. During the deposition of the concretionary member, the amount of $MgCO_3$ evidently became inadequate, for some parts contain only 3.07–8.36 per cent of $MgCO_3$ (equivalent to 6.82 and 18.56 per cent of dolomite, respectively). This deficiency of $MgCO_3$ permitted the formation of the nearly pure limestone beds that contain the most abundant and most perfectly developed of the concretionary forms. In the dolomite marls, the excess $CaCO_3$ was available for the formation of concretions. No positive figures for the proportion of dolomite to calcite are available, but Garwood[2] gives an estimate of two parts of matrix (dolomite) to one part of concretions (calcite). This ratio would give an original composition of 52.49 per cent dolomite and 30.56 per cent limestone, plus the other constituents of the rock.

The pure limestone is undoubtedly a chemical precipitate like the dolomite. A mechanical analysis of the powdery dolomite showed, according to Trechmann,[3] that 99.45 per cent of the particles would pass through a 240-mesh screen. It is probable that both the dolomite and limestone were originally in this finely divided state, and that subsequently the calcareous material was crystallized.

The beds overlying the concretionary member are dominantly dolomitic, become increasingly gypsiferous, and finally pass upward into the Red Beds. Thus, we note that the character of the sea water had changed in that the sulfate radical replaced the carbonate radical in combining with the calcium not needed to form the double salt dolomite.

In summing up, it is believed that the dolomites and limestones of this formation are direct chemical precipitates, and that when-

[2] E. Garwood, *Geol. Mag.*, Vol. VIII (1891), pp. 435–40.

[3] C. T. Trechmann, *Quar. Jour. Geol. Soc.*, Vol. LXX (1914), pp. 232–65.

ever, as occurred during the formation of the limestone of the concretionary member, the quantity of $CaCO_3$ became sufficiently abundant essentially pure limestone was deposited. Both rocks were originally extremely fine-grained carbonate muds, a physical condition that made possible the development of the concretionary structures.

THE CONCRETIONARY STRUCTURES

Many names have been given to the great variety of concretionary structures. These names include "coralloid," "discoid," "stalactitic," "cellular," "spherical," "cannon ball," and "oölitic"; but all the forms can be placed in two major groups, which are, in the order of their abundance, (1) columnar and (2) globular. The columnar forms could be subdivided on the basis of the position of the columns, i.e., whether they are vertical, inclined, or radial; and the globular concretions could be classified as to whether they are massive, crystalline, or fibrous; but there appears to be little value in this further subdivision.

Both types of concretionary structures are found throughout the 250 feet of the concretionary limestone, although they are far more abundant in a series of beds about 80 feet thick. The columnar type is found chiefly in the limestone beds containing only 5–20 per cent of dolomite, and the globular forms occur primarily in the dolomite marls.

COLUMNAR FORMS

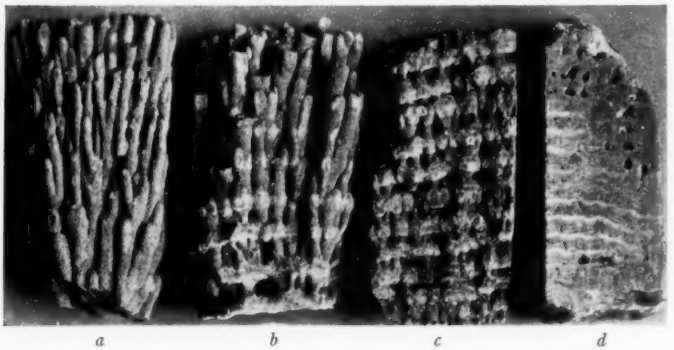
The columnar forms are not actually concretions, but rather rods of limestone ranging in diameter from a small fraction ($\frac{1}{16}$) of an inch up to 2 inches and in length from 1 to 6 inches (Figs. 2 and 3). The vast majority of them are less than $\frac{1}{2}$ inch in diameter and from 1 to 3 inches in length. Rods of the larger diameters have rounded elliptical forms.

The rods may consist of single fibers of calcite, as in a few fibrous types; but much more commonly they are composed of fine-grained limestone (Fig. 1.) The rods may be single or branched; have a parallel or radial arrangement; and interlock with or overlap one another. The position of the rods relative to the bedding planes may be perpendicular, inclined at all angles, or parallel (Figs. 1, 2, 3, and

4), or they may be entirely independent of the bedding planes. It is the intergrowth of these various types of rod aggregates that produces the beautiful structure of Figure 1. Throughout the formation

a *b* *c*

FIG. 2.—Three common types of columnar forms. Note the calcite crystals covering the rods; also the evidence of the original bedding planes. Specimens collected by the author in the Fulwell quarries, Sunderland, in 1925. Natural size.



a *b* *c* *d*

FIG. 3.—Another group of columnar forms showing gradations from branching rods to an almost solid aggregate. Specimens collected and photographed by George Abbott in 1908. Photograph loaned the author by the Sunderland, England, Museum. About natural size.

bedding planes, if present in the original rock, are present in the concretionary structure whatever its position.

The rods are generally massive on the interior, except where bedding planes pass through them. The exterior of the columns may be

smooth or covered with crystals of calcite, many of which show rhombohedral or scalenohedral faces (Fig. 2). Calcite crystals exceeding $\frac{1}{32}$ inch in length are usually fibrous (see left side of Fig. 2*a*). Such fibers are perpendicular to the *surface of the rod*. The skin of calcite crystals is most common on the relatively independent rods (Fig. 2*a*).

FIG. 4.—Concretionary bed of magnesian limestone showing bedding planes (cross bedding in lower part) and inclined rods. Specimen collected by the author at the Fulwell quarries. About natural size.

Some columns show a concentric banding, wherein the curvature is toward the outer end of the column or rod, whatever its position, and is always at right angles to the length of the rod (Figs. 3*b* and *c*, and 5). As will be seen later, this banding is a part of the larger concretionary structure.

The rod aggregates assume various forms and have probably been called "concretionary structures," because many of them show a series of concentric layers analogous to the assumed essential struc-

ture of a true concretion (Figs. 1 and 5). Some aggregates consist of a series of essentially independent parallel rods (Fig. 4). Rods that have coalesced form aggregates like those in Figures 2 and 3. Another type of structure (Fig. 5) still possesses a rodlike character, but the individuality of the rods has been largely lost. In some of the aggregates composed of simple rods, the rods are only rudely paral-

FIG. 5.—So-called "honeycomb" type of concretionary structure, showing the concentric growth and the union of adjacent columns along these lines of growth as well as original bedding planes. Specimen collected by author in the Fulwell quarries, Sunderland. Natural size.

lel (Fig. 4); in aggregates consisting of branched rods, some of the branches lie adjacent and nearly parallel to the parent rod, although most of them diverge and give rise to radiating groups (see Figs. 2*a*, 3*a* and *b*).

The most common type of columnar aggregate is composed of concentrically united rods. Practically all of the structures seen in Figure 1 are the result of the union of columns (shown in Figs. 3*c* and 5). The columns have united along the well-defined bedding planes;

otherwise their union has followed the concentric banding (Fig. 5). Columnar aggregates may develop in any position, i.e., above or below a bedding or joint plane, or on either side or both sides of an inclined joint. The result of this development in the much jointed limestone of the concretionary member is an intricate maze of concentric aggregates. In essentially all of the structures, the rods or columns are separated by areas of fine, buff-colored dolomite powder. This powder is readily washed or blown away when the rock is broken. It should be noted that the limestone of the columns also contains small amounts of this dolomite powder.

The columnar types are undeniably associated with the bedding planes and joints or fissures of the limestone. The position and direction of these divisional openings within a bed do not affect the development of the structures. Where the bedding planes are largely laminations or poorly developed, as they are in the cross-bedded layers, the concretionary structures cross them (Fig. 4). A divisional opening sufficiently large, however, to permit the passage of ground water may become the seat of a concretionary structure. There are several sets of intersecting joints, which makes the possibilities for the development of these structures almost infinite in number, and their pattern very complex. Variability in porosity and composition of the rock further increases the complexity of pattern.

GLOBULAR FORMS

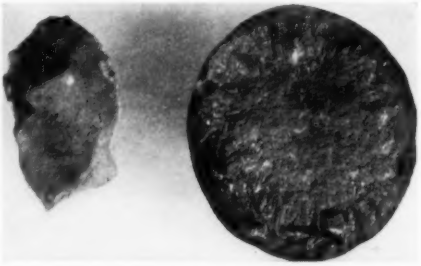
The globular forms are more nearly typical concretions than the columnar forms, in that they possess a somewhat spherical form, though not all of them have an internal concentric arrangement. The globular forms are commonly known as "cannon balls," a name fairly descriptive of their size and shape. They range in size from tiny globules that might well be called "oölites" to spheres $3\frac{1}{2}$ feet in diameter. The majority of the forms, however, are a foot or less in diameter (see Fig. 6). The small concretions are very numerous, which shows that there were many centers of crystallization.

Most of the globular forms are massive, showing no evidence of an internal structure, although on their exterior even these forms may show traces of bedding planes (see Fig. 6). Other forms are composed of both coarsely crystalline calcite and dense material, and

still others consist of radial calcite fibers. A few forms show concentric banding (Fig. 7). Any globular concretion may have a thin coating of fibrous calcite, the fibers of which usually end in crystal faces.

FIG. 6.—Spherical concretions of calcite from the dolomite marl that show the bedding planes on their exterior. Specimen collected by author from Fulwell quarries, Sunderland.

The massive forms are composed chiefly of finely crystalline, brownish to buff, or even gray calcite (limestone) and buff or yellow dolomite intimately intergrown. The coarsely crystalline calcite of some of the concretions is generally a brownish to gray color and breaks with curved faces. It usually forms the outer part of the concretion, but may be on the interior with the dense limestone on the outside. Small cavities in some concretions contain calcite. The radial forms average 4–6 inches in diameter and are composed of calcite fibers which show rhombohedral cleavage (Fig. 7*b*). In the few concentrically banded forms noted, the banding was poorly developed (Fig. 7*a*). It might start in the center of the concretion or at some other point. A few of the forms have a cavity in the center, and a few have a nucleus.



a *b*

FIG. 7.—Internal structure of two spherical concretions. *a* shows concentric banding, and *b* a massive interior and radial exterior. Specimens collected by author from Fulwell quarries in 1925.

Some of the globular forms are more nearly ellipsoidal than spherical, and some have developed nodes. Other variations in shape have

been produced by the coalescence of two or more concretions (Fig. 6). The internal structure of a concretion is unaffected by its shape. All types of the concretions may occur in the same bed. Where one concretion has been pressed against another, stylolitic seams have developed.

Analyses of the massive concretions show that they contain from 10 to 32 per cent of dolomite. The smaller the amount of dolomite present, the harder are the concretions. The fine-grained powder around the concretions is nearly pure dolomite, analyses by Trechmann[4] showing 87–90 per cent. The concretions composed of coarsely crystalline or fibrous calcite consist, of course, of nearly pure calcite, as does also the fibrous calcite shell found on all types of concretions. The scalenohedral crystals of calcite occurring in the dolomite marl are also pure calcite. Small areas of pure, white calcite occur rarely in some of the larger fibrous forms.

Most of the concretions show, either in the interior or on the exterior, some evidence of the original bedding planes. These planes are not equally well developed in all of the concretions, and they may be lacking in those of the very massive phases of the marl. If bedding planes were present in the marl in which the concretion formed, they are preserved in the concretion. All the bedding lines observed pass *through the concretions*. Bedding planes may or may not be present in the powdery dolomite.

Globular concretions, unlike the columnar forms, occur independently of the divisional openings. Likewise, they have no definite horizontal or vertical distribution within the marl beds. The controlling factors in their mode of occurrence are the porosity and composition of the marls. The best-developed concretions occur in two well-defined horizons of the dolomite marls of the 80-foot section. Both large and small concretions may occur within a single bed. The smaller concretions usually occur in the beds that consist of approximately one-half dolomite powder.

ORIGIN OF THE CONCRETIONARY STRUCTURES

PREVIOUS VIEWS AS TO ORIGIN

The extraordinary development of the concretionary structures in the magnesian limestone has long attracted students of geology.

[4] *Op. cit.*

Their occurrence was noted as early as 1817 by Winch.[5] The first important discussion of their origin was by Adam Sedgewick[6] in 1826 and 1827, although his report was not published until 1835. He described the various forms in detail and gave their composition. Sedgewick believed that the structures were formed after the deposition of the beds, and that their formation was due to the crystallization of calcite that had been separated from the dolomite. He did not think any material had been added to the bed.

In a very valuable paper published in 1891, Garwood[7] discussed the prevalent views as to the origin of these structures. He effectively disposed of the view that the $CaCO_3$ was introduced into the beds subsequent to their deposition, and advanced good evidence to prove that the concretionary structures resulted from the aggregation of the $CaCO_3$ that was originally mingled with the dolomite. Jukes-Brown,[8] in commenting on Garwood's paper, suggested that the $CaCO_3$ in solution above a bed which had already been deposited on the sea floor passed down into the bed below and was deposited. Garwood[9] later showed that the presence of the bedding planes was opposed to this view. Abbott[10] discussed the form of these concretions, but added nothing to the discussion of their origin. Courty[11] regarded the concretions as being due to the aggregation of the $CaCO_3$. Trechmann,[12] in 1914, published a fine series of analyses of the concretions and associated dolomite marls. He expressed the view that the greater part of the dolomite in the Magnesian limestone was a chemical precipitate.

Woolacott,[13] in 1919, thoroughly discussed these structures. He be-

[5] N. J. Winch, *Trans. Geol. Soc. London*, Vol. IV, p. 9.

[6] *Ibid.*, 2d. ser., Vol. III (1835), p. 89.

[7] *Op. cit.*

[8] A. J. Jukes-Brown, *Geol. Mag.*, Vol. VIII (1891), p. 528.

[9] *Ibid.*, 1892, p. 44.

[10] G. Abbott, *Naturalist* (1895), p. 233; also *Geol. Mag.* (1901), p. 35; and *Proc. Geologists' Ass'n.*, Vol. XXVII, Part 3 (1916), p. 192.

[11] G. Courty, *Bull., Geol. Soc. France*, Ser. 4 (1906), pp. 482–83.

[12] *Op. cit.*

[13] D. Woolacott, *Geol. Mag.*, Vol. VI (1919), pp. 452–65, 485–98.

lieved that the Magnesian limestone consisted dominantly of chemically precipitated sediments. Some of the dolomite, the marl, for example, he thought was due to chemical deposition; other dolomites he regarded as being due to the subsequent union of $CaCO_3$ and $MgCO_3$, which is a very improbable reaction. He accounted for the higher calcite content of certain beds by the process of breaking up the dolomite molecule with a $CaSO_4$ solution (a reaction that is highly improbable and certainly unproved) and leaching out the $MgCO_3$. The liberated $CaCO_2$ was then redeposited as concretionary forms. Other statements regarding the solution of the various salts (notably $MgCO_3$ in the presence of $CaCO_3$) involved are at variance with common geochemical knowledge. Woolacott's paper is especially valuable for its able presentation of the general geology of the Magnesian limestone formation.

PROPOSED EXPLANATION

Although the same fundamental process is responsible for the development of both the columnar and globular structures, the steps in the formation of the two forms are different, and therefore the origin of each type will be discussed separately. All the structures, however, represent perhaps the most remarkable example of controlled recrystallization shown by sedimentary rocks anywhere. This control was due to the presence of numerous divisional openings in a porous rock of unusual mineral composition. Before taking up the detailed discussion of the origin, the events preceding the formation of the concretionary structures will be discussed.

The dolomites and limestones are believed to have been dominantly chemical precipitates, in keeping with a common type of sedimentation occurring in the Permian seas. In an enclosed sea, limestone is deposited first, or as long as $CaCO_3$ is more abundant than $MgCO_3$. If the magnesian salts are increasing in concentration, as they were in this Permian sea, the excess of magnesium in the presence of an ample amount of the carbonate radical eventually causes the precipitation of the double salt, $CaMg(CO_3)_2$, dolomite. Both the calcite and dolomite of these formations were deposited as extremely fine-grained muds (as the dolomite marls prove). Slow deposition resulted in numerous bedding planes and laminations; whereas more

rapid precipitation produced thicker massive beds containing little evidence of bedding or lamination.

The great abundance of limestone in the section containing the major part of the concretionary structures indicates that for a period of time the amount of $CaCO_3$ contributed to the sea was large and, as a result, much calcite (but always with some dolomite) was deposited. Although these formations are called "limestones," most of them contain from 10 to 30 per cent of dolomite. Likewise, the dolomite marls contain from 5 to 40 per cent of calcite. All these materials are crystalline, but extremely fine grained, though not so fine grained as most of the Chalk.

This fine-grained mixture of dolomite and calcite became sufficiently consolidated by pressure (not by cementation or crystallization) to develop joints as the series of beds became involved in earth movements. Should such a condition be questioned, it is only necessary to recall the hundreds of feet of Chalk beds which show this type of physical consolidation. The powdery dolomite present in the marly beds proves that cementation has *never* occurred in them. Whether the presence of the rigid bryozoan reef may have influenced the development of joints during consolidation is an open question. At any rate, the *age* of the concretionary structures, especially the columnar ones, proves positively that the jointing preceded their formation. It was the development of the numerous intersecting joint systems or divisional openings that made possible the first step in the origin of the structures.

ORIGIN OF THE COLUMNAR STRUCTURES

The extreme variability in form of the columnar types and their vertical, inclined, or radial position have been noted, as has likewise their occurrence singly or in various aggregates and their undeviating association with the divisional openings of the rock. The columnar forms may be above or below a bedding plane or on either side of a joint plane, whatever its position. A structure composed of single rods or aggregates of straight rods developed *outward* from the divisional openings, *but always at right angles to the joint or bedding plane.* During the development of the spherical or ellipsoidal forms, the columns grew radially outward from these bedding planes, as is

shown not only by the position of the outer surface but by the *concentric lines* within each column.

How did recrystallization produce these remarkable columnar aggregates? Why did rods and columns result rather than a solid bed of limestone? The answer to these questions lies in a triple relationship of joints, porosity, and composition. The development of a series of adjacent rods or columns will be outlined first, as all the other forms are aggregates of such rods or columns.

When the two salts, $CaCO_3$ and $CaMg(CO_3)_2$, are mixed in a porous aggregate, such as the original carbonate mud of this formation,[14] the $CaCO_3$ is much the more soluble substance. That $CaCO_3$ was taken into solution is amply proved by the *abundance of pure fibrous calcite* in the concretionary structures. That the dolomite powder is relatively more insoluble and that it was not taken into solution is proved by the *abundance of dolomite powder* within and around all these structures. Water, therefore, was the agent of recrystallization—not water that was flowing rapidly through the rock mass, but water that was penetrating the porous rock and slowly but surely recrystallizing the calcite grains into a limestone.

Where joints were abundant and open, more water could enter the beds—a factor that influenced the growth of certain types of structures. Thus, the divisional openings were an essential factor in the recrystallization of the rock mass. The water, in moving along the bedding or joint planes, came in contact with the mixture of $CaCO_3$ and $CaMg(CO_3)_2$. This water, containing some CO_2, as all ground waters do, dissolved the *most minute* grains of $CaCO_3$ from the mixture. The movement of the water, however, was not fast enough to remove the dissolved $CaCO_3$, which was therefore *redeposited upon the larger grains of calcite* in the mixture of calcite and dolomite. The carbonate radical liberated by the deposition of the $CaCO_3$ was thus again available to assist in dissolving other molecules, so that the total amount of CO_2 present need never have been large. Dolomite was more or less intimately mixed with the calcite; and, as the calcite grains grew larger, some of the dolomite was included within the

[14] Some men have contended that the original magnesian salt was $MgCO_3$, which is an improbability; but, even if it had been, $MgCO_3$ is still more insoluble relative to $CaCO_3$ than is $CaMg(CO_3)_2$.

recrystallized area and a part was excluded and pushed ahead of the crystallizing aggregate. Centers of recrystallization would start anywhere and everywhere, depending upon the porosity and composition of the mass. Areas rich in calcite grains would grow fastest; and, if they were associated with material that was relatively more porous, growth would be further aided as more material could thus be carried to the point of deposition. If the composition and porosity of the mass were uniform, the columns should be similar in size and shape. Many of the aggregates indicate that this was true. Continued upward growth of a rod was made possible by the formation of a passageway for the water in the segregated powdery dolomite between the growing columns. That cementation occurred before the movement of any material, save that in solution, is shown by the fact that the bedding planes pass through adjacent columns undisturbed. Bedding planes are present in all of the concretionary structures that developed in beds containing them. If the quantity of dolomite was large, the columns were far apart; if small, the columns developed close together and commonly united, especially along the growth lines. Union of columns may also be the result of growth along a thin bed. The dolomite powder is still present between the columns in the undisturbed concretionary structures.

Massive columns bearing only the inherited bedding planes and, more rarely, successive growth lines apparently formed rapidly. After the major growth of the column had ceased, the slow addition of calcite to the outside formed the pure, fibrous calcite skin, so common throughout the structures. Branching occurred where a portion of a rod received material slightly faster than did adjacent parts (owing to variations in porosity and composition); and, once started, the growth of a branch followed the manner of growth of the adjacent rods. Thus, the action of ground water on a porous medium of two minerals of different solubilities resulted in the formation of the columnar structure.

Where growth took place along a bedding plane, the columns could form on one or both sides as the controlling factors of porosity and composition determined. In the specimens studied, the majority were below the bedding planes, as they were, also, below the inclined joints. The common position of the columns at right angles to

the divisional openings is due to their outward growth from these openings, and the inclined position of some of the columns is evidently due to a deviation in direction of the movement of the water caused by differences in the porosity of the rock. A series of parallel or nearly parallel columns gave rise to the parallel aggregates.

The radial aggregates developed wherever the water spread outward from a local point along a plane. The joint planes were not absolutely smooth surfaces, and anywhere along them wider spaces where *more* water was present might have occurred. This resulted in the entrance of a greater amount of water into the adjacent material; and, if the joint in most directions from the opening was tight, the water in spreading outward would have a spherical or an ellipsoidal surface. Columns would then develop as under the other conditions; but, since the solutions were moving outward radially, the columns would be radial. Radial aggregates developed also because of the branching of the rods, a condition influenced by the porosity and composition of the material. In the radial aggregates, the concentric growth lines are usually continuous over the entire surface, showing that the outward movement of the water was uniform. Spherical forms were especially likely to develop at the intersection of two or more joint planes. In studying the figures accompanying this article, it should be remembered that the third dimension is not shown, and it is this dimension that enables one to see why the radial columns developed. Breaking columns apart in the field shows this clearly.

Summary.—The growth of all columns starts from divisional openings in the rock, and development is controlled by the porosity and mineral composition of the rock mass and the character of the ground water. The very complex structure consisting of multitudes of columnar aggregates results from the interplay of the various factors that control the development of a single column.

ORIGIN OF THE GLOBULAR FORMS

The origin of the globular concretions is much simpler to work out than is that of the columnar forms. Divisional openings played no part in the origin of these forms, save as they furnished passageways

for the introduction of water from adjacent beds into the powdery dolomite.

The massive concretions formed wherever there was sufficient calcite to act as a nucleus or initial point of deposition. The steps of growth were identical with those taking place in the formation of a column. The ground water deposited calcite about a group of larger grains, and thus the concretions were started. Growth was very steady and uniform for the great majority of forms, as is evidenced by their massiveness. The movement of material must have been slight, as any original bedding lines in the rock were preserved in the concretions. If there was a local abundance of calcite, large concretions 6–12 inches in diameter were formed; if the calcite was widely distributed, a large number of small concretions resulted. The porosity of the mass also influenced the size of the concretions; the more porous the rock the larger the concretions, as a rule. During growth, concretions commonly interfered with one another, and occasionally two or more united. An irregular addition of material resulted in nodes on the concretions. The usual outer layer of fibrous calcite on the massive concretions represents the final slow addition of calcite.

There are two types of globular concretions that contain crystalline calcite: radially fibrous and coarsely crystalline concretions. The fibrous concretions are the result of slow growth starting from many points and usually ceasing with the formation of crystal faces on the ends of the fibers. As the fibers grew, essentially all of the dolomite powder was pushed ahead of the growing crystal. The coarsely crystalline concretions (usually massive concretions with only the outer part coarsely crystalline) originated in a similar manner but, because of the porosity of the surrounding material, did not assume a fibrous form. The individual scalenohedrons of calcite found in the dolomite marl had an origin similar to that of the other two crystalline forms.

SUMMARY

The concretionary structures of the Permian Magnesian limestone near Sunderland, England, occur in a series of dolomites and

limestones that are believed to be dominantly chemical deposits. After the physical consolidation of these fine-grained carbonate muds occurred, they were fractured and jointed, which permitted the ready entrance of ground water. This water brought about the recrystallization of the calcite adjacent to the divisional openings and thus developed the series of columnar forms that constitute the columnar concretionary structures. The globular concretions of the dolomite marls are the result of a similar growth within the powdery dolomite.

DISCUSSION OF PALEONTOLOGICAL CHRONOLOGY

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Bulletin No. 80 of the National Research Council, *The Age of the Earth*, goes beyond its title, as might be expected, and stimulates interest in the whole field of geological chronology. It brings up to date the important methods of inquiry. In view of this, it seems a pity that one of these methods, that of "paleontological chronology," receives no great recognition. Possibly this is because it bears so much more on the practical question of measuring geologic time than on the ultimate, and at present rather futile, problem of the age of the earth.

Paleontological chronology was originally conceived by the genius of Henry Shaler Williams, and was developed very largely by the labors of S. S. Buckman. Williams started on an assumption (a wild one for the time) that sedimentary thickness is no indicator of the lapse of time. This has since been thoroughly justified through the principles of grade and baselevel. Sediments cannot accumulate above grade.[1] They cannot be indefinitely preserved above baselevel. Hence, the thickness accumulating in any place is governed by the amount of subsidence; which, in a certain time, may be much or little. A secondary factor is the supply of sedimentary material; and, therefore, knowing the irregularly intermittent nature of diastrophism which underlies both factors, how absurd it is to suppose that any particular kind of sediment has a characteristic rate of accumulation! Or, even that there are such things as rates at all connected with the process of sedimentation, the word "rate" implying necessarily some form of steadiness in movement or acceleration!

The whole thing is to be compared to the results of two laborers, the one piling and the other scattering. Their efforts tend to balance each other, if they work equally hard and on solid ground. But, if

[1] The conception of grade is, of course, quite as applicable to wave as to river transportation.

the pile is made on sinking ground, the piler has an advantage, for though the pile does not gain in height, the material is gathered in and sinks down to where the scatterer's efforts cannot reach it. The advantage may not be permanent, for the sinking ground may hold up for a time. Then, again, the scatterer may redouble his exertions. To this the piler may reply by increasing his. Under such conditions the growth of the pile will be anything but proportional to the time employed in making it.

For like reasons, the accumulation of the sedimentary rocks is too irregular a process to form a product sufficiently even grained to serve as a basis for chronology. Williams saw this. He saw that a few feet of strata at one place correspond to thousands of feet at another. That all stratal successions are full of gaps, most of which are invisible, and that none is of known dimensions. He saw that faunal changes are not always or necessarily coincident with lithic changes. Furthermore, that the so-called sedimentary record is really two records: one of sedimentation; and, mixed with it, yet quite distinct and independent, the other, that of the successive steps in the evolution of life. He concluded that the rock record, even though it had long provided a rough sort of episodic chronology, must be divorced from the chronological scheme. On the other hand, the evolution of living things provides in its successive steps the successive *dates* of a real chronology, requiring only to be gathered and systematized into a single universal frame.

Williams insisted, therefore, on what he called a "dual nomenclature"—geographic names for rock deposits and the rough periods marked by such deposits, and fossil names for exact time divisions. This phrase "dual nomenclature" seems to have done more harm than good, for it led geologists to suppose that Williams was trying to make two separate nomenclatures for two aspects of the same thing, and hence to regard his attempt as superfluous. On the contrary, his two nomenclatures apply to utterly different things: on the one hand, the concrete record of rocks; on the other, the abstract chronology of evolution.

In detail, the time units of Williams' system were the periods of duration of the successive faunules—purely abstract in the basis and not susceptible of exact delimitation. But what matter? The

essential thing, knowledge of the true succession of time units, is obtainable very definitely from the vertical succession of fossils in the strata. Thus, the foundation of the chronology is concrete and plainly obvious.

Buckman seems to have evolved these ideas independently, and to have built upon them considerably. He suggested the term "hemera" for the indivisible time unit. He changed the emphasis somewhat by disregarding faunal equilibrium (which Williams used as the basis for separating faunules) in favor of short-lived species. His hemera is the time of culmination of one, or several contemporary, species. Working on Munier-Chalmas' discovery of the possibility of non-contemporaneity among fossils associated in one bed, he showed that many beds contain the mixed record of several successive hemerae which can be separated and placed in their true order only by observation of a better-amplified record. He then proceeded to trace such mixed records into places where the non-contemporary species occur not mixed, but in superposed succession.

The application of this principle, coupled with a keen discernment of differences between species, led Buckman, in his work on the Jurassic System, to an elaboration of the chronology undreamed of by Williams, and as yet unparalleled in any other system except the Cretaceous to which Spath has latterly extended Buckman's principles. There is an urgent need to extend these principles farther, so as to chronologize the whole fossiliferous column. And, furthermore, there is a need to check the results by comparative work in parts of the globe remote from those where the chronologies are first established. The problem is still in its infancy. It requires the support of keen thinkers and rugged workers everywhere to bring the results to the stage of completeness and exactitude where their universal value will enforce allegiance and understanding.

The paleontological chronology has, as I see it, three main uses. In the first place, it is the continuous, graduated frame on which we may arrange our knowledge of geologic history, and with which we may correlate local records in order, in the words of Powell, to develop geologic history as a consistent whole. It serves the same purpose to the geologist as the years to the historian. The hemerae are our dates. By correlation with them, the most trivial of events are

placed in their true temporal relations, thus making it possible to illuminate here and there the evidence of causation.

The correlation just mentioned is a second and independent use. A chronological frame built upon the successive lives of fossil species contains in itself the evidence for correlation. Then, again, the whole problem is simplified by reference to a standard in place of the inexact method of the matching of strata. And besides, how much easier is it to become familiar with a system known in terms of names than one in which each subdivision requires so many relative terms such as "upper" and "lower," "later" or "earlier"? To give an example: Once the system is known, how much more meaningful is "Stepheoceratan age" than "late lower Middle Jurassic." No one could guess that *his* meaning of "late lower Middle Jurassic" was the same as *mine*, but of "Stepheoceratan" there can be no question.

To this advantage of definiteness we may add accuracy of expression. A certain bed, *A*, at one place is shown to be contemporaneous with a bed, *B*, at another, since both are correlative with the *sauzei* hemera. Later, it may be shown by other means that they are not synchronous, but successive deposits. Hemeral correlation, in the first place, involved no inaccuracy, because only contemporaneity was predicated, whereas any statement of stratal equivalence would necessarily, as most statements of stratal equivalence now do, involve a minor element of falsity. The case is comparable to one in history of two famous events both dated as 1910, yet they occurred not together, but successively, and almost half a year apart.

In this connection let it be noted clearly that there can be no stratal "equivalent" of a hemera, because hemerae cannot be exactly delimited. In an incomplete development of strata, it may be possible to say positively that these 10 feet of beds, with diastems above and below, belong entirely to the *sauzei* hemera. But they are not its equivalent because above and below are erosional interruptions. Trueman's "epibole" was founded in disregard of this, and hence, if it is to be used as a stratigraphic term, requires redefining. In a complete development of strata, if such exist, occurs the equivalent of a hemera, but since it has no defined limits, none can pick it out. The limits of a hemera are unknowable.

I have always found this simple limitation puzzling to students.

Perhaps my favorite parable will elucidate it. Suppose we could distinguish no period shorter than a year in everyday life. It would then be quite impossible to discern the point where one year passed into another. We should then celebrate New Year only by rejoicing with the coming of Spring. But such a limitation would not in the least decrease the accuracy of our only time unit, the year, for historical chronology. For instance, 1492 would still mark the time when Columbus touched the island outposts of North America, fourteen hundred and ninety-two summers after the birth of Christ.

Lastly, the paleontological chronology, when it is sufficiently perfected, will serve as a scale to measure geologic time. For most purposes, the scale may be left in its primary form. However, the question arises of the length of hemerae in terms of years. This becomes the more pressing when we feel the need of comparing past with present. There are, of course, considerable difficulties in the way of doing this, and close limitations beyond which we may never be able to go. Nevertheless, it seems likely that much may be done through checking hemerae against approximate time in years obtained from various methods, especially those dependent on atomic disintegration. Then, different hemerae may be expected to have different durations in terms of years, and, of course, the duration of a single hemera is non-mensurable. Even so, it is to be confidently expected that when the ground is sufficiently covered we shall know the approximate duration of hemerae in years, and may be able to say that such-and-such a deposit or structure was formed in so many years, or that a certain ancient process proceeded at the rate of so many feet a year.

AVAILABLE RELIEF AND TEXTURE OF TOPOGRAPHY
A DISCUSSION

DOUGLAS JOHNSON

ABSTRACT

Attention is directed to a recent discussion of "available relief," "critical relief," and "drainage relief." Suggestions are made for redefining two of the terms, and the factors involved in the development of relief are examined. It is shown that the spacing of drainage lines (texture of topography) is a factor of prime importance in determining quantity of relief, and that available relief may vary greatly with differences in stream spacing. Finally, it is suggested that stages of landmass dissection should be expressed in terms of the amount of *mass* removed, not in terms of amount of upland *surface* destroyed; and it is concluded that available relief offers a useful basis for judging the progress of landmass removal.

INTRODUCTION

In a recent paper on "Available Relief as a Factor of Control in the Profile of a Land Form," Waldo S. Glock[1] makes suggestions which deserve careful consideration. "Available relief" is defined by this writer as "the vertical distance from an original, fairly flat upland down to the initial grade of the streams." "Critical relief" is that amount of available relief (said to be commonly 200–300 ft.) "at which valley flats begin to form just as the upland disappears." "Drainage relief" is the vertical distance through which rain water "moves over the ground from the time the water first strikes the surface until it joins a definite stream."

In discussing these elements Glock concludes that available relief is an important "factor of control" in the development of surface profiles; that low available relief (up to 150 ft.) permits the existence of upland flats and valley flats at the same time; that high available relief (anything above critical relief, say above 200–300 ft.) prevents the simultaneous existence of upland flats and valley flats, the former being destroyed before the latter begin to form; that available relief must be an influential factor in the preservation of uplifted peneplanes; and that the surface profile of a region with low available relief "ill becomes the common conception of the geographic cycle."

The present writer has found in Glock's paper a stimulus to more

[1] *Jour. Geol.*, Vol. XL (1932), pp. 74–83.

careful analysis of certain problems, and ventures to discuss some of the matters treated by him. As is perhaps inevitable in such a discussion, greater stress is laid upon differences of interpretation than upon points concerning which agreement renders discussion less profitable.

When studying Glock's paper the reader must weigh several questions of prime importance. Is available relief really a "factor of control," or is it merely the result of more important controlling factors? Will different streams in a given locality attain the graded condition at the same horizontal level, as pictured in Glock's diagrams; or is the profile of each graded stream concave upward, with valley flats rising gradually, but with increasing slope as one ascends the main river, and rising with ever increasing rapidity to higher and still higher levels up the tributaries? Does the existence or nonexistence of upland flats depend primarily on the amount of available relief, or must we take account of other factors of equal or greater significance?

DEFINING AVAILABLE RELIEF

We have seen that Glock measures available relief from "an original, fairly flat upland down to the initial grade of the streams." The conception promises to be useful, but as presented it involves difficulties. Many dissected landmasses (e.g., tilted fault-block mountains) never possessed an original, fairly flat upland; yet we must take account of available relief in these as well as in plains and plateaus. The word "original" raises a question as to whether it is intended to exclude distinct and far-spreading uplands developed by peneplanation, or by stripping of soft beds from a resistant stratum. The "grade" of a stream is a condition rather than a thing to which measurements can be made, and must be distinguished from the "gradient" which all streams possess whether or not they have attained the graded condition. Furthermore, the *initial* position at which a stream becomes graded is not a determinable quantity. We may determine the level at which a particular part of a given stream is now graded; in certain cases we may find traces of former levels at which it was graded; but in no case can we be sure that any of these represent the position at which the initial grading took place. A narrow valley with the first beginnings of a continuously graded valley

floor is never proof that this is the first level at which these conditions existed. Where streams are not now graded, we cannot foretell with any accuracy the level at which this balanced condition will be attained.

A further difficulty arises from the fact that a stream is not graded simultaneously throughout its course, or progressively from the mouth toward the head. Many parts become graded, and open out valley flats of considerable width, only to lose these features as ungraded reaches are propagated upstream. Must a stream be graded from the locality under inspection clear to the sea before we can measure available relief? Or shall we be content with a graded reach stretching for several miles through the region investigated? And shall we measure from the upland to the largest graded stream dissecting the region, or to the nearest adjacent graded stream? And to what part of this stream shall we measure—to the lowest graded portion or to the nearest? We may assume, I suppose, that only streams engaged in dissecting the landmass are to be considered; for it would obviously be improper to measure available relief from the upland of a block mountain to the level of a graded stream flowing through a neighboring lowland quite outside the block itself.

At least some of our difficulties will disappear if we rephrase Glock's definition to read as follows: *Available relief is the vertical distance from the former position of an upland surface down to the position of adjacent graded streams.* If the stream has a well-graded stretch for some miles through the particular area involved, it becomes the basis for calculating present available relief, even though future available relief, when the stream is graded to the sea, may be notably greater. This appears to be the method followed by Glock, and seems the only practicable procedure. Any other method would compel us to examine a stream from a given locality clear to the sea before we could ascertain the available relief in the locality under investigation. Examining a single topographic quadrangle would not avail, for the stream graded there might have ungraded reaches farther downstream on the adjacent or more remote quadrangles; and where areas downstream remained unmapped, available relief could not be determined save by extended field reconnaissance. The method suggested not only avoids these practical difficulties, but seems to

be fair from the theoretical point of view; for our ultimate aim is to determine whether a given amount of incision will permit upland remnants and graded valley flats to exist simultaneously. The principles involved would seem to be the same whether the stream is graded locally with reference to a hard-rock barrier or another stream or lake, or graded throughout with reference to the sea.

If no streams in the region are graded, the level at which they will probably become graded may roughly, but not accurately, be estimated in case the drainage is tributary to graded streams in a neighboring region. If the distance to the nearest graded member of the same drainage system is great, available relief for the area studied can scarcely be ascertained.

TEXTURE OF TOPOGRAPHY AND AVAILABLE RELIEF

To the elements "structure, process, and stage" in a geomorphic description there must be added "texture" if a complete picture of a landmass is to be secured. "Texture" may be defined as the average size of the units composing a given topography. The term is usually employed in describing regions dissected by stream erosion, and then depends on the spacing of drainage lines, whether or not these are represented by perennial streams. Where drainage lines are closely set the texture is said to be fine; where they are widely spaced, with large undissected interstream areas, the texture is coarse. Texture is not an expression of quantity of relief, steepness of slope, or stage of development. We may have coarse texture in regions of low or high relief; in regions of steep or gentle slopes; and in young, mature, or old stages of a landmass. It is merely one aspect, although a very vital one, of drainage pattern.

Texture of topography depends on a number of variable factors, among the most important of which are climate and rock composition. For reasons which need not here be reviewed, a semi-arid climate generally tends to produce a finer texture in the maturity of a landmass than does a humid climate, although main streams may be more widely spaced and hence the texture be very coarse in youth. Fine-grained, poorly consolidated rocks, other things being equal, give a finer texture than do rocks made up of large fragments or of small particles firmly consolidated. Stage of development affects

texture, since maturely dissected landmasses tend to have more, and more closely spaced, drainage lines than do youthful or old landmasses; although under certain conditions a mature region, with as close-set drainage lines as it will ever witness, may have a very coarse texture. Whether a landmass is in its first or a later cycle of erosion will affect its texture, the first cycle tending to have the coarser texture at a given stage of youth or maturity. Other factors enter into the problem; and especially when one studies apparent texture as represented on maps must the scale of the map and the accuracy of the mapping receive careful attention.

Now it will at once be apparent that whether or not a given amount of stream incision in any landmass will destroy the upland surface must in the first instance depend primarily upon texture. If the streams are very closely set, they cannot cut deeply before their spreading valley walls will undermine the last remnants of the upland. If the streams are widely spaced, they may cut very deeply before their spreading valley walls intersect and so reduce the upland to sharp-crested ridges. To be sure, rock character plays a secondary rôle by determining how rapidly valley walls will weather back to a given angle of slope, and hence how rapidly they will consume the upland. Rapidity of stream incision affects the problem in a somewhat similar manner. Both these last factors affect the stage of valley development at a given phase of stream incision; and there must be added the effects of valley widening by lateral planation if consideration is to be given to the progressive consumption of upland flats after streams have been graded.

Other factors affect the removal of upland surface, among which must be reckoned the depths to which the streams will intrench themselves; and here available relief enters, since it measures the maximum limit of the first rapid intrenchment. In a region uplifted but slightly, streams can incise themselves but slightly; and if streams are widely separated they will attain the graded condition and open out valley flats before the upland flats have been seriously reduced in area. With greater uplift, these same streams might, by carving deep canyons with flaring sides, consume all the upland surface long before attaining the graded condition. Hence, depth of stream intrenchment is a factor which must be considered in associa-

tion with stream spacing. But it should be remembered that whether the available relief is great or small, the upland may completely disappear long before that relief is attained, in case the texture is fine. If any one factor among the several which determine the preservation or destruction of upland remnants is to be singled out as most critical, texture, or the spacing of the drainage lines, would seem marked for that rôle. Yet we must doubt whether it is practicable to speak of a "critical texture" when it is but one of the several factors determining the final result.

VARIATIONS OF AVAILABLE RELIEF AND PRESERVATION OF UPLAND SURFACES

If the reader will recall his own field observations, and will consult a range of topographic maps wider than that represented by sheets from the moderately elevated and humid coastal plain, prairie plains, and Appalachian Plateau, he will find it difficult to attach definite importance to any particular amount of available relief in relation to the amount of upland surface preserved. Glock appears to have sensed this difficulty, for he observes that his statements "are generalized and averaged; they probably do not apply universally to all specific areas"; and he further notes that "an arid climate apparently increases the amount of relief which is critical for the simultaneous existence of two flats." The reader may recall, as does the writer, fine-textured topography in artificial deposits of clay, mine dumps, and other accumulations subjected to natural weathering and erosion, in which the entire original upland surface had been consumed by an intricately ramifying natural drainage system, although the available relief was to be measured in terms of a few feet only. He may recall "badland" topography in natural clay formations in the humid east, and in marl deposits of the arid west, in which the available relief did not exceed 25, 50, or 100 feet in areas where the upland was completely removed and only sharp-crested ridges survived. Apparently there is no available relief so low but that the upland flats may be gone in case the texture is sufficiently fine.

On the other hand, there is no available relief so high but that upland remnants may still be conserved provided the texture—because of climate, rock character, youth of dissection, or other cause—is suf-

ficiently coarse. Let the reader examine first some areas in the humid Appalachian Plateau where character of rock and character of climate combine to give a fairly coarse texture. Toward the north the Pocono (Pa.) quadrangle with an available relief of something like 800 feet has great areas of well-preserved upland. In the south the Scottsboro (Ala.) and the Chattanooga and Sewanee (Tenn.) quadrangles all show much upland. The first has available relief of nearly 1,000 feet, while in the last two the available relief exceeds 1,000 feet. In the central Appalachians the Beckley (W.Va.) quadrangle shows much upland where the New River seems just beginning to widen its valley floor and available relief measures from 1,000 to 1,200 feet.

Passing to the humid Ozark Plateau, we find on the Marshall (Ark.) quadrangle much upland preserved with available relief varying from 600 to 1,200 feet. In humid New England where disordered crystallines have been peneplaned and later uplifted and dissected, we find on the Hawley (Mass.) quadrangle remnants of the peneplane upland where the available relief varies from 1,100 to 1,400 feet. The Pocono and Hawley quadrangles are believed to be pertinent, although it is fully recognized that areas subject to former glaciation cannot be regarded as conclusive, because of the possibility that abnormal drainage conditions in a particular valley may have produced abnormal results.

Compare with the foregoing some examples from the arid west. The Hell Creek (Idaho) quadrangle shows a meandering stream on an open valley floor 500 feet below a well-preserved upland surface. It is obvious that the stream could here reduce its graded floor until the available relief is greatly increased before the upland remnants are consumed. On the Arlington (Ore.) quadrangle the John Day River has opened out a valley flat for many miles below a well-preserved upland. The present available relief is 700 feet, and is subject to early increase, as the river is tributary to an ungraded stretch of the Columbia. In New Mexico the Tucumcari quadrangle shows upland remnants where the available relief varies from 500 to over 1,000 feet. The Gallina (N.M.) quadrangle has much upland where the available relief reaches 1,600 feet, while on the Pullman (Wash.) quadrangle an irregularly dissected upland is just beginning to be attacked in the new cycle due to deep incision of the Snake River.

Along a graded section of the river the present available relief in places exceeds 1,700 feet.

In this connection it is interesting to examine the Colorado River where it is locally graded for several miles through the weaker rocks of the Algonkian wedge. Here (Vishnu quadrangle, Ariz.) the present available relief can be determined for only a very restricted area, and perhaps it is not fair to cite it in our discussion. But since the present available relief locally exceeds 5,000 feet, and the upland surface of the plateau is well preserved over vast areas on either side, it is interesting to speculate on how much farther the river may intrench itself and then open out a long-continuous valley flat before the last remnants of the upland disappear. It would seem clear that under favorable conditions available relief may exceed 5,000 feet while upland remnants and valley flats simultaneously exist. Among such favorable conditions are great elevation above baselevel, rapid stream incision, wide spacing of streams, and rocks resistant to erosion in a given climate (e.g., limestones in an arid region).

It should not be understood that Glock ignores the various factors we have mentioned above. At the end of his paper on available relief there is a section on "Relationships," the first sentence of which reads: "The simultaneous existence of two flats is chiefly a function of climate, available relief, rock and structure, and the distribution of streams." The influence of rapidity of stream incision is recognized in a short paragraph devoted to "rate of land elevation," while climate is further considered in a preceding section labeled "Discussion." But since these factors are only briefly treated toward the end of Glock's paper, and find no place in his main discussion of available relief as a factor of control, the reader might gain the impression that available relief is itself a causal factor which can properly be treated alone. If the considerations presented above have validity, the case is really far less simple.

AVAILABLE RELIEF AND THE CYCLE

In Glock's paper one encounters criticisms of current ideas concerning the geomorphic or geographic cycle which should be welcomed, but carefully pondered, before the implied alternatives are embraced. Reference is here made to but one such criticism. He

states that "the surface profile of a region with low available relief ill becomes the common conception of the geographic cycle." In the discussion leading up to this conclusion Glock does not distinguish between maturity of the landmass and maturity of streams dissecting the landmass. Since the two are neither directly related nor necessarily synchronous, one needs to know how Glock determines the maturity of each, and what is his conception of the cross profile of a mature river, and of a mature plain or plateau, before one can compare his views with those of others. Certainly the particular figure which he cites as ill becoming the common conception of the cycle fits perfectly into its proper place in the cycle of plain dissection as developed by Davis.

Possibly Glock is interpreting stages of development in terms of the amount of upland surface removed or remaining. This is not an uncommon procedure, but it leads to inevitable difficulties. The amount of the landmass removed, rather than the amount of upland surface removed, would seem to be the vital factor in landmass dissection; and the same proportion of a given landmass may be removed with much upland remaining in one case, none in another.

DRAINAGE RELIEF

"Drainage relief" is defined at the beginning of Glock's paper as "the vertical distance through which rain water moves over the ground from the time the water first strikes the surface until it joins a definite stream." But when that author comes to discuss drainage relief a few pages later he seems to make it the distance from the upland or ridge-crest level down to the valley-botton level. Since water passing from ridge crests to valley bottoms usually descends as small but definite streams formed well up toward the crests, it must be clear that there is a wide difference between these two conceptions of drainage relief. In humid regions, to which Glock restricts discussion, it is doubtful if rain water often passes over the ground through any great distance before joining some definite stream. Movement is for the most part underground, or on the surface in tiny rills and larger streams which are definitely fixed though far from being graded. Only locally, and for short distances, does the water descend as a surface sheet before being gathered into larger or

smaller streams. Perhaps the relations Glock desires to emphasize can best be expressed in terms of topographic relief as ordinarily defined—the distance from principal ridge crests to principal valley bottoms.

If Glock would revise his definition of "drainage relief" to make it correspond with his later usage, and his definition of "available relief" as suggested in an earlier section of the present discussion, both terms would seem to be useful additions to geomorphic terminology. Drainage relief would then become the topographic relief developed by incision of streams in a landmass, and could thus be distinguished from the relief resulting from mountain uplifts, volcanic eruptions, and other causes. The drainage relief of a given locality would be measured directly from the existing level of the higher peaks or ridges in that locality, vertically downward to the level of the adjacent deeper valleys. In contrast, the available relief of a given locality would be measured from the former position of the upland surface down to the position of adjacent graded streams. Each type of relief would have to be determined locally, for each could vary widely over moderate distances. Since the upland in a given locality might be completely consumed, and no stream in that locality might yet be deepened to the level at which it could grade its course, it is evident that available relief could not there be measured, whereas drainage relief could be so measured.

STAGES OF LANDMASS DISSECTION

The matter is of importance in connection with the problem of stages of landmass dissection. As indicated above, the writer has found that attempts to express stages of landmass dissection in terms of amount of upland removed lead one into insuperable difficulties. If much upland surface represents youth, then little or none should represent old age. Yet parts of the intricately dissected Allegheny Plateau, with no upland remaining, are for sound reasons commonly accepted as the type of a mature plateau. Furthermore, one plateau may under favorable conditions be in large part reduced to a peneplane, yet have scattered monadnocks with summits preserving appreciable expanses of the original upland; while another very much less eroded, like those portions of the Allegheny Plateau just referred

to, have no upland preserved. The first case represents an old plateau, yet has upland remnants; the second plateau is only mature, but has no upland.

On the principle that proportion of mass removed is the only true criterion of stages in the dissection of that mass, I have long been accustomed to determine the stages of evolution of plains, plateaus, and mountains in terms of the amount of erosion of the mass. It is sufficiently obvious that in calculating proportion removed and proportion remaining one should not envisage the mass as extending from the upland surface to the center of the earth. It is less obvious, but I think equally true, that one should not compute the original mass of the land form as extending from the upland surface to the plane of sea level. The reasons for excluding from consideration the mass lying deep in the earth's interior and a certain portion lying above sea level are the same; they are not properly part of the mass included within the land form as ordinarily developed, and are not subject to removal by dissection in the period required to reduce the landmass to a peneplane. In other words, they are not parts of the "available" mass.

In the case of a plateau located in the interior of a continent, stream erosion may reduce the mass to a peneplane, and old age thus be attained, while the surface of the country is still some hundreds of feet above sea level. It is true that if the land remains quiescent for the enormously long period required, the peneplane may be lowered, with infinite slowness, until much of the remaining mass is removed; but during all this time the surface form will retain the characteristics of old age. To include in the mass subject to stream dissection that underlain part of the earth's crust which is not normally available to dissection is a procedure that seems theoretically of doubtful value, and from the practical standpoint leads, especially in the case of plains originally low, to obvious absurdities in determining stages of dissection.

In estimating the amount of landmass removed, some other plane of reference than sea level is required. I have been accustomed to estimate roughly the progress made by stream erosion and weathering in removing the mass between the upland surface, or where this is gone, the higher ridge crests, and a plane passing through the bot-

toms of the deepest valleys. In other words, drainage relief as redefined above has alone been considered, although this has happened to coincide with available relief in those cases where upland remnants remain and valley bottoms are graded. I am inclined to think that available relief, whenever this can be determined or even roughly estimated, would form a sounder basis for judging progress of landmass removal. Certainly it has more claim to attention on theoretical grounds, and in formulating this conception Glock has made a distinct contribution to geomorphology.

The thoughtful critic may point out that what Glock has called "available relief" is not the total relief actually available, nor even that which may actually be developed in the progress of a cycle. Streams often grade their courses on levels well above that to which they later reduce their valley floors even during their maturity. The criticism is theoretically sound, yet it does not seem to the writer to invalidate the practical usefulness of the conception. If a plateau were young if only 49 per cent of its available mass were removed, and mature if 51 per cent had been eroded, then indeed the fact that graded valleys are further deepened, the fact that available relief cannot be directly measured or even estimated with any degree of accuracy where the upland is completely removed or valleys not yet graded, and the further facts that streams are not usually graded throughout their courses either simultaneously or progressively headward, and when graded are at different depths below the upland in different places, would introduce formidable difficulties.

But when stages of landmass dissection are only roughly divided into three principal stages; when anything from 30 or 40 per cent removal to 60 or 70 per cent removal gives the characteristics we have been accustomed to associate with maturity of the landmass; when these estimates are only quick and rough approximations, made in a glance of the eye over the terrain or over the topographic map; and when such crude approximations give all the useful results needed to convey the fact that a landmass is in an early stage, an advanced stage, or a late stage of its evolution, the difficulties which in theory loom large in practice disappear.

Experience shows that with such conceptions of youth, maturity, and old age of landmasses as have been outlined above, competent

students do not differ greatly in classifying groups of maps according to stages of development of the landmasses represented; probably no more than they would differ if highly refined but difficultly applicable methods of classification were attempted. After all, it is the broad, major conception with which the geomorphologist is chiefly concerned. He can, apparently, secure the best general conception of stage of landmass dissection by using as a basis of quick computation "available relief" wherever this is measurable, or subject to approximate estimation. Where available relief is indeterminable, he may fall back on "drainage relief," making allowance for large or small error according as available relief in the region may possibly be greatly in excess, or only moderately in excess, of drainage relief.

THE SHAPES OF ROCK PARTICLES: A DISCUSSION

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I was much interested in the paper on the "Volume, Shape, and Roundness of Rock Particles" by Hakon Wadell in a recent number of the *Journal of Geology*.[1] The paper contains a valuable discussion of the geometric properties mentioned in the title and contributes a number of useful definitions. Certain statements and assumptions in it, however, seem to me to be susceptible of more than one interpretation and are the basis of the following comments.

The ratio of the surface area[2] of a sphere of equivalent volume to the surface area of the particle in question, is defined as the degree of true sphericity, and is truly regarded as an important factor in the behavior of such a particle in suspension. It seems to me that the surface ratio stated, which reaches the maximum value of unity in a sphere, is only one of the unique geometrical properties of a sphere and cannot be regarded as the exclusive measure of true sphericity. I have listed elsewhere two other unique properties of spheres.[3] These are constant curvature over the entire surface and constant diameters in all directions through a common center.

Wadell discusses, moreover, a solid having "a maximum degree of roundness," but which is "not a sphere."[4] Later he defines an index of roundness based on the curvature of corners as an additional measure of shape and thus further discriminates between sphericity and roundness. I believe that most geologists regard "sphericity" as a precise term, indicating exactly the shape of a sphere and possession of the properties of minimum surface area in relation to volume, of constant diameters through a common point, and of constant curvature over the whole surface in all directions. Degree of curvature is a measure of approach to this end-condition and can be measured in

[1] Vol. XL (1932), pp. 443–51.

[2] *Ibid.*, p. 445.

[3] C. K. Wentworth, "A Laboratory and Field Study of Cobble Abrasion," *Jour. Geol.*, Vol. XXVII (1919), pp. 513–14.

[4] Wadell, *op. cit.*, p. 446.

at least three ways, by reference to the three properties of spheres mentioned above. The noun "spheroid" is applicable to shapes nearly, but not quite, spherical and especially, but not necessarily, to figures of revolution. "Roundness" is a term which may properly be more loosely used, with reference both to solid shapes and plane figures. But when applied to a solid in the phrase "maximum degree of roundness," most will agree that a perfect sphere is implied. Wadell's conventionalization of the surface area ratio as the epitome of sphericity is carried to still less acceptable extremes when he states[5] that a solid may "have a high degree of sphericity and no roundness."

I infer from his previous definitions that in this statement he really means that a particle may contain near the maximum possible volume in relation to its surface area and still be rugose or angular on its surface. This is quite true; unfortunately, in actual practice, much more true than is suggested by his use of a dodecahedral garnet crystal as an example. In further discussion of this point it is stated by Wadell that "a cylinder terminated at each end by a half sphere cannot become more rounded." If one accepts Wadell's definition of roundness in strictness, this is true; but in the light of a more general concept of roundness such a statement is subject to challenge, not only on geometrical grounds, but also with reference to natural or experimental shape modifications.

Neither when attacked by mechanical wear or by solution do surfaces of one curvature which merge with other surfaces of lesser curvature, in any direction, or of lesser average distance from the center of the mass, retain their uniformity of cylindrical or spherical curvature. It might be supposed that a cube, subjected to random wear or solution, would have its edges reduced to cylindrical forms and its corners to spherical forms, with curvatures decreasing with time, and with reduced, but still plane, faces. However, this is not the case. Those parts of the once plane faces adjacent to the near-cylindrical edges are subject to more loss than the centers of the faces, and the parts of the near-cylinders adjacent to the faces are subject to less loss than its other parts. The resulting form is a smooth transition in curvature from the minimum of the face to the maximum of the edge or corner at any given stage and in the course of time there is a pro-

[5] *Ibid.*

gressive decrease in the deviation of both curvatures and diameters from a mean, as well as an increase in the index which Wadell has called the "degree of true sphericity."[6] Wadell's capsule shape would be similarly modified.

This behavior in the case of wear is due in part to the decrease in ratio of supporting volume to area of attacked surface from the less convex faces to the more convex edges or corners of the cube, or from the one-way convex cylinder of Wadell's capsule to its universally convex end, and also to the greater protrusion of the more convex parts of either solid. In the case of solution it is due to the increase of the ratio of cross section through which diffusion may take place, to the area of attacked surface. The removal of dissolved material and hence the relative capacity of the liquid for further solution is governed by relationships closely analogous to, if not identical with, the well-known potential theory. According to each of these sets of conditions of attack, Wadell's capsule, as well as a partially rounded cube, will continue further shape modification toward a spherical form. If roundness be measured solely in terms of the radius of curvature of the most convex part, as implied by Wadell, then the capsule will not be further rounded in the earlier stages of the process postulated; but I believe few will wish to so restrict the idea of roundness. By analogy it would appear that the solid figure with curved surfaces, inscribed in a given right parallelopiped, and having the least surface area per unit of volume, is a triaxial ellipsoid. The sphere, inscribable in a right parallelopiped of equal side, i.e., a cube, is a special case of such an ellipsoid, where the axes A, B, and C are equal. Further data on the shape modifications of cubes in abrasion are given in Figures 27 and 28 of the paper previously cited.

Emphasis given by Wadell to the difference between roundness and compactness (perhaps the best word for what he calls "sphericity")[7] is a point very well taken and of much importance in the study and classification of sedimentary particles, but it appears to me to be veiled in an unfortunate terminology. His method of designating roundness by relating the radius of curvature of corners and edges to the radius of the largest inscribed circle is a contribution to the grow-

[6] Wentworth, *op. cit.*, Figs. 8–25.

[7] Wadell, *op. cit.*, pp. 449–51.

ing number of indexes of roundness and of shape.[8] It should be recognized that no one of these methods can be heralded as "correct"; each has been elaborated by an individual worker in response to a certain practical problem, and all are more or less conventional and adapted to convenience of measurement. Perhaps no one of them will ever become "standard," and at present it would be a misfortune if such standardization should take place.

Too many processes are involved and too many points of beginning and ending are involved to permit the stage of shape evolution always to be measured by one index. Rather, it is to be hoped that more investigators will apply self-devised quantitative numerical methods to their problems, while profiting by the successes and failures of their predecessors. In the measurement of roundness, the most fundamentally sound reference values, such as surface area, the value of the mean of random radii or of random curvatures, have been found in my own experience to be difficult or onerous of measurement, so much so that simpler and less perfect measures have been adopted.

In this connection, and in conclusion, I am eager to learn what method is advocated by Wadell for the measurement of the surface areas of either large or small particles in the numbers necessary to obtain valid averages. It is recognized that the paper under discussion is intended to be mainly theoretical and that another paper is forthcoming. In view of the importance of the surface area ratio, long recognized, description of laboratory methods for making this and other related measurements with reasonable economy of time is awaited with interest.

[8] Wentworth, *op. cit.*, pp. 513–17; also *U.S. Geol. Surv. Bull. 730-C* (1922), pp. 93–97; *U.S. Geol. Surv. Prof. Paper 131-C* (1922), pp. 75–83; *Wash. Univ. Studies, Sci. and Tech. Ser., No. 5* (1931), pp. 28–32; E. P. Cox, *Jour. Paleon.*, Vol. I (1927), pp. 179–83; A. C. Tester and H. X. Bay, *Sci.*, Vol. LXXIII (1931), pp. 565–66; also *Jour. Sed. Petrol.*, Vol. I (1931), pp. 3–11.

SPHERICITY AND ROUNDNESS OF ROCK PARTICLES

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The purpose of my article "Volume, Shape, and Roundness of Rock Particles," which appeared in the *Journal of Geology*,[1] was to secure priority and to announce that a more comprehensive work had been prepared for publication.[2] This preliminary article is the subject of criticism by Dr. C. K. Wentworth in this number of the *Journal*. Because of this criticism it seems advisable to go further into the problem and also to consider critically that part of Wentworth's own research work which is the foundation for his opinion.

WENTWORTH'S SHAPE DETERMINATIONS

Wentworth states his opinion that the surface ratio (sphericity), as I have used it, is ". . . . only one of the unique geometrical properties of a sphere and cannot be regarded as the exclusive measure of true sphericity." He also points out that he has listed elsewhere[3] two other properties of a sphere. These unique properties, as he calls them, are ". . . . constant curvature over the entire surface and constant diameters in all directions through a common center." The earlier statement to which Wentworth refers[4] is as follows:

> In studying this change in shape it is necessary to recognize varying degrees of roundness, in other words, to have a numerical answer to the question of how round a given piece is. At least three criteria of roundness readily occur to one in considering the question. These are (1) the ratio of surface area to volume, (2) the average deviation of diameters from a mean diameter, (3) the average deviation of convexities from a mean convexity. Each of these values or coefficients of roundness reaches a minimum in the case of a sphere and has a maximum in the case of a line or plane without volume.

[1] Vol. XL (1932), p. 443–51.

[2] A typewriten copy is now available in the University of Chicago Library (Geological Section).

[3] Chester K. Wentworth, "A Laboratory and Field Study of Cobble Abrasion," *Jour. Geol.*, Vol. XXVII (1919), pp. 507–21.

[4] *Ibid.*

It is possible that the ideas expressed by Wentworth in the present number of the *Journal* may have been the foundation for his statement quoted above, but there is quite a difference between the meanings of the quotations from the two papers. Wentworth's first point, "the ratio of surface area to volume," is only the third type of *specific surface* as suggested by Wo. Ostwald[5] in 1909. A solid can have every possible shape and still have the same ratio of surface area to volume. A cube with a surface area of 6 square centimeters and a volume of 1 cubic centimeter has a specific surface of $\frac{6}{1}=6$, just as the sphere inscribed in the same cube (surface area of sphere = 3.1416; volume = 0.5236; specific surface = 6).

How Wentworth can obtain any reasonable numerical value for shape according to his second and third points is a puzzle which he has never solved. He proposed a "practical solution of the problem." Before discussing its value, it will first be shown that his first and second points are actually expressions for the *mean deviations* of the diameters and the convexities respectively.

There are several types of averages, such as the arithmetic mean, the geometric mean, the harmonic mean, etc. Wentworth failed to

[5] Wo. Ostwald, *Grundriss der Kolloidchemie* (1909). On page 84 the third type of the specific surface is expressed as the ratio:

$$\frac{\text{Absolute Oberfläche eines einzelnen Teilchens der dispersen Phase}}{\text{Volume eines einzelnen Teilchens der dispersen Phase}}.$$

There is a confusion in the literature in respect to the *specific surface*, because of a superimposed meaning given to this term by Fritz Paneth in 1922. According to him, the specific surface is the size of the surface area of one gram of the solid. This meaning of the specific surface is for sedimentological purpose less suitable because the density of the material enters as a factor.

Fritz Paneth, "Über Eine Methode zur Bestimmung der Oberfläche Adsorbierender Pulver," *Zeitschrift für Elektrochemie und Angewandte Physikalische Chemie*, Vol. XXVIII (1922), pp. 113–15.

It should be noted that Jackson, in 1903, proposed the term "surface factor," the numerical value of which is supposed to express the total surface of the particles in a given weight of dry material. In 1905 Purdy changed the definition, the *surface factor* being the relative surface of the particles per unit volume. When dealing with a single particle, I believe that Ostwald's term *specific surface* has been more generally adopted.

W. Jackson, "The Calculation of the Fineness of Ground Materials," *Trans. English Ceramic Soc.*, Vol. III (1903), pp. 16–22.

Ross C. Purdy, "The Calculation of the Comparative Fineness of Ground Materials by Means of a Surface Factor," *Trans. Amer. Ceramic Soc.*, Vol. VII (1905), pp. 441–47.

state which kind of average he had in mind. It is here assumed to be the arithmetic mean, the simplest and the one most commonly used. The first part of his second point, i.e., "the average deviation of diameters" thus becomes synonymous with the *arithmetic mean of the deviations of the diameters;* and the entire second point receives the following form: *The arithmetic mean of the deviations of the diameters from a mean diameter.* This is the expression for the *mean deviation* of the diameters. Accordingly, the second and third of Wentworth's points may be simplified to *the mean deviation of the diameters* and *the mean deviation of the convexities* respectively. Since the numerical value of the mean deviation of a group of items depends upon the numerical values of these items, it follows that the mean deviation cannot be used as an expression for shape. For the sake of simplicity, assume that the three diameters of a solid of a given shape are 1, 2, and 3 centimeters; thus the mean deviation equals 0.666. . . . . For a larger solid of exactly the same shape and of proportionally larger diameters, 10, 20, and 30 centimeters, the mean deviation is 6.666. . . . . The same holds for the mean deviation of the convexities. Thus it is evident that the mean deviation cannot be used, because the numerical value supposed to express the shape is influenced by the size of the solid (0.666 and 6.666). Furthermore, I have already shown that the diameters are out of the question as factors for comparison of solids of different shapes.[6]

Wentworth decided upon a "practical solution of the problem." To that end he used "the ratio of radius of curvature of the most convex part of the surface to half of the longest diameter through that point." An analytical discussion will show that the problem of expressing the shape of a solid is not solved by this method. As will be shown later, Wentworth's error rests actually upon his confusion of roundness and shape. The most convex parts of a geometric cube are its solid corners.[7] Being a geometric solid, these corners are infinitely sharp, and their radius of curvature consequently equals

[6] Wadell, *op. cit.*, pp. 443–44.

[7] In my larger paper I have used the expressions *plane corner* and *solid corner*, the former defined as every such part of the outline (of a plane area) which has a radius of curvature equal to or less than the radius of curvature of the maximum inscribed circle of the same area; a *solid corner* is understood to be the intersection of three or more converging surfaces of a solid.

zero. Thus, according to Wentworth's shape determination, there is to be measured the longest diameter through one of the solid corners of the cube, i.e., the diagonal. If the diagonal of the cube is d, the shape of a geometric cube, according to Wentworth's formula, is $\frac{2\times 0}{d}=0$. It is evident that the same value will be obtained for all shapes of geometric solids, the most convex part of which is a solid corner (radius of curvature equals zero).

That Wentworth's idea of the shape of a solid is not clear is further shown by his later papers, in which he elaborates upon the *flatness ratio*, an invention for the purpose of expressing the shape of beach pebbles.[8] The flatness ratio is expressed by $\frac{D'+D''}{2D'''}$, i.e., the average of the length and breadth divided by the thickness. Accordingly, a sphere has the same flatness ratio as a cube.

It has been shown above that those "unique properties," if used for expressing shape in the way described by Wentworth, are of doubtful value. That the diameters are out of the question as factors in shape determinations has been discussed. It is also impossible to

[8] Chester K. Wentworth, "Shapes of Beach Pebbles," *U.S. Geol. Surv., Prof. Paper 131-C* (1922), pp. 75–83.

Besides papers already mentioned, the following articles deal with suggestions made by geologists for the determination of shape of rock particles. In addition a recent paper by Wo. Ostwald is included.

J. Hirschwald und J. Brix, "Untersuchungen an Kleinschlagdecken behufs Gewinnung einer Grundlage für die Prüfung der naturlichen Gesteine auf ihre Verwendbarkeit als Strassenbaumaterial," *Bautechnische Gesteinsuntersuchungen* (Mitteilungen aus dem Mineralog.-geol. Inst. d. K. Techn. Hochschule Berlin) (1918), p. 39–49; Chester K. Wentworth, "A Method of Measuring and Plotting the Shapes of Pebbles," *U.S. Geol. Surv. Bull. 730-B* (1922), pp. 91–102; "A Field Study of the Shapes of River Pebbles," *ibid., 730-C* (1922), pp. 103–14; A. C. Trowbridge and M. E. Mortimore, "Correlation of Oil Sands by Sedimentary Analysis," *Econ. Geol.*, Vol. XX (1925), pp. 409–23; E. P. Cox, "A Method of Assigning Numerical and Percentage Values to the Degree of Roundness," *Jour. Paleon.*, Vol. I (1927), pp. 179–83; J. E. Lamar, "Geology and Economic Resources of the St. Peter Sandstone of Illinois," *State Geol. Surv. Ill., Bull. 53* (1927), pp. 44–46, 148–51; A. Pentland, "A Method of Measuring the Angularity of Sands," *Proc. and Trans. Roy. Soc. Can.*, Ser. 3, Vol. XXI (1927), App. C, Titles and Abstracts, p. xciii; Allen C. Tester, "The Measurement of the Shapes of Rock Particles," *Jour. Sed. Petr.*, Vol. I (1931), pp. 3–11; F. G. Tickell, *The Examination of Fragmental Rocks* (Stanford University Press, 1931), pp. 6–7; Wo. Ostwald, "Ueber difforme System. I. Stereometri und Systematik difforme Systeme," *Kolloid-Zeitschrift*, Vol. LV, Heft 3 (1931), pp. 257–72.

get a numerical expression for the shape of a solid with reference to "the constant curvature over the entire surface" of a sphere. All geometric solids, including geometric configurations of crystals, which are composed of plane faces, solid corners, and edges, will accordingly have the same shape, because the radii of curvature of the corners and edges equal zero, and the radius of curvature of the plane faces is infinitely large. Finally, none of the "unique properties" suggested by Wentworth is a maximum property. The maximum property (*Maximumeigenschaft*) of a sphere is an isoperimetric property. Further discussion on this subject appears in a later paragraph.

THE DEGREE OF SPHERICITY OF A PARTICLE

Wentworth criticises my term "degree of true sphericity." He expresses his belief ". . . . that most geologists regard *sphericity* as a precise term, indicating exactly the shape of a sphere."

The *Oxford English Dictionary*[9] says that "sphericity" meaning a spherical body is obsolete. The *Century Dictionary* gives "sphericity" as the character of being in the shape of a sphere and as synonymous with the French *sphéricité*. *Nouveau Larousse* gives under the word *sphéricité* an example: "La sphéricité de la terre n'est pas absolue." Thus it is evident that sphericity is a quality or character and as such is expressible in various degrees. The formula which I have suggested for expressing the degree of sphericity leads to a numerical value which expresses the shape-character of a solid relative to that of a sphere of the same volume and specific gravity, and on the basis of the isoperimetric property of the sphere.

The geometric foundation for the concept "degree of true sphericity" rests upon the isoperimetric property of a sphere. The first attempt to prove that among all solids of a given surface area the sphere had the greatest volume was made by Lhuilier.[10] J. Steiner[11]

[9] James A. H. Murray, *A New English Dictionary* (Oxford, 1914–15).

[10] S. A. J. Lhuilier, "De relatione mutua capacitatis et terminorum figurarum, geometrice considerata; seu de maximis et minimis. Varsaviae 1782." Not seen. Reference obtained from: Ernst Steinitz, "Polyeder und Raumeinteilungen," *Encyklopädie der Mathematischen Wissenschaften*, Bd III, 1^2, AB 12 (1914–31), p. 38.

[11] K. Weierstrass, *Jacob Steiner's Gesammelte Werke* (Bd. I [1881]; Bd. II [1882]), Vol. II, p. 304.

simplified the proof and drew up the following thesis: "Unter allen Körpern von gleichem Inhalte hat die Kugel die kleinste Oberfläche; und unter allen Körpern von gleicher Oberfläche hat diesselbe den grossten Inhalt." Further proofs were furnished by H. A. Schwarz[12] and H. Minkowski.[13] Wilhelm Blaschke[14] gave a neat analytical formulation of the isoperimetric property of a sphere. He called this property the *Maximumeigenschaft* ("maximum property") of a sphere.

In suggesting a formula for obtaining a numerical value expressing the shape of a solid, I was aware that the geometricians have made a number of more or less successful attempts to classify solids, especially the convex polyhedron. The geometrician is, however, interested in such a classification for the sake of geometry. He is not concerned with the physico-chemical origin of the solid or the way its properties interact with external forces. As previously pointed out, I am not interested in all kinds of geometric properties, but only in those which are of sedimentological importance. Nevertheless, I have shown in the preceding pages that, basing my sphericity formula on the "maximum property" of a sphere, the concept "degree of true sphericity" has also a strictly geometric foundation. My formulas for sphericity and roundness are also applicable to all kinds of solids, including geometric. All geometric solids composed of plane faces, corners, and edges have a roundness value of zero, thus in accordance with the views expressed in my preliminary paper in respect to unworn solids, exhibiting non-rounded, sharp corners. The roundness is independent of the sphericity, which is expressed by the ratio of the surface area of a sphere of the same volume as the solid to the actual surface area of the solid.

The shape of solids (in physico-chemical meaning) has been discussed in many different fields of science, but nowhere have I found any clue as to the principal factor involved in the term. Both the so-called *volume-weight* and *sediment-volume* are to no small extent

[12] H. A. Schwarz, reference not seen. See Blaschke below.

[13] Hermann Minkowski, "Ueber die Begriffe Länge, Oberfläche und Volumen," *Jahresbericht der Deutschen Mathematiker-Vereinigung*, Vol. IX (1901), pp. 115–21.

[14] Wilhelm Blaschke, "Kreis und Kugel," *Jahresbericht der Deutschen Mathematiker-Vereinigung*, Vol. XXIV (1915), pp. 195–207.

influenced by the shape of particles. The minimum porosity of quartz sand, composed of particles of approximately the same size, is influenced by the shape of the granules.[15] It is generally assumed that the shape of the particles plays an important rôle in ore treatment and in respect to the properties of cement, concrete, pigment powder, and other industrial products. Gilbert in his classic work, "The Transportation of Débris by Running Water,"[16] has given considerable attention to the shape of rock particles in transportation and accumulation of the débris. It is generally assumed that the shape of particles influences the settling velocities. Freundlich[17] has expressed the opinion that, since Stokes' law only holds for spherical particles, its invalidity can often be considered as a proof for the non-spherical form of the particles. The shape of particles influences probably the capillary rise of water in sand.[18] P. Curie[19] said, "Lorsque le corps se déforme, l'énergie en volume est constante et l'énergie totale varie proportionnellement à la variation de surface." An interesting discussion on shape of coal particles in respect to combustion has been given by Rosin and Kayser.[20] Consideration is given to the consumed volume in proportion to the surface area of fire attack. Terzaghi,[21] discussing the elasticity properties of soils with respect to the presence of "gedrungenen Kornes" (apparently high degree of sphericity) and "Schuppen" (lamina-shaped particles), has pointed out that the contact points of the latter are at least a hundred times greater than for the same size of particles of "gedrungenen" form. Eilh. A. Mitscherlich discussed the

[15] J. E. Lamar, *op. cit.*

[16] G. K. Gilbert, "The Transportation of Débris by Running Water," *U.S. Geol. Surv., Prof. Paper 86* (1914).

[17] Herbert Freundlich, *Kapillarchemie* (1932), Bd. II, p. 82.

[18] Alfred Mitscherlich, "Untersuchungen über die physikalischen Bodeneigenschaften," *Landwirtschaftliche Jahrbücher*, Vol. XXX (1901), pp. 361–445.

[19] P. Curie, "Sur la formation des cristaux et sur les constantes capillaires de leurs différents faces," *Bull. de la Société Minéralogique de France*, Vol. VIII (1885), p. 146.

[20] P. Rosin und H.-G. Kayser, "Zur Physik der Verbrennung fester Brennstoffe," *Zeitschrift des Vereins Deutscher Ingenieure*, Vol. LXXV, (1931), pp. 849–57.

[21] Karl v. Terzaghi, "Festigkeitseigenschaften der Schüttungen, Sedimente und Gele," *Handbuch der Physikalischen und Technischen Mechanik* (edited by F. Auerbach und W. Hort), Bd. IV, Part 2 (1931), p. 516.

compactibilty (not compactness) of particles. "Die Gestalt des Hohlraumvolumen des Bodens wird bedingt durch die Gestalt der einzelnen Teilchen und durch die Art der Aneinanderlagerung."[22] ". . . . das Hohlraumvolumen um so verzweigten ist, je grösser die Summe der Oberfläche der festen Bodenteilchen ist."[23] Mitscherlich, however, did not give any definition of shape, though he apparently saw some relationship between shape, volume, and surface. *What, then, is the principal factor involved in the shape of a solid?*

It may be taken for granted that none of the writers on the sedimentological significance of shape have had the geometrician's classification of solids or the crystallographer's definition of form in mind. It seems, on the basis of the quotations in the preceding paragraphs, that there should be some kind of relationship between volume, surface, and the sedimentological shape. These three factors are, together with Steiner's thesis,[24] the basis for the following:

1. Solids of equal surface areas and equal volumes have the same shape.

2. Solids of equal surface areas but of different volumes have different shapes.

3. Solids of equal volumes but of different surface areas have different shapes.

We have, then, two ways of expressing the sedimentological shape, either on the basis of equal surface areas but different volumes or by equal volumes but different surface areas. I have chosen the latter because it is for sedimentological purposes more convenient. In view of the truly unique, isoperimetric property of a sphere and with regard to the extraordinary rôle this shape plays a natural phenomena, it was adopted as a standard shape for comparison. Maintaining the third proposition above as a foundation, the numerical value of the shape-character of a particle was then expressed with reference to a sphere by the ratio of the surface area of a sphere, of the same volume as the particle, to the actual surface area of the particle; thus

$$\frac{s}{S} = \text{Degree of true sphericity,}$$

[22] Eilh. A. Mitscherlich, *Bodenkunde für Land- und Forstwirte* (1905), p. 84.

[23] *Ibid.*, p. 86. [24] Quoted on p. 315.

where s is the surface area of a sphere of the same volume as the particle, and S is the actual surface area of the particle. The value obtained is independent of the size of the particle, and thus dimensionless.

Variations of the crystallographic habit do not indicate any change in the real crystallographic form of a crystal. In the same way, two solids of the same sedimentological shape, or rather the same degree of true sphericity, may have a different appearance, i.e., they may vary in respect to surface details, roundness of the corners, and non-metric properties such as the number of corners and edges present or in respect to the relative position of the plane surfaces.

Though the word *shape* has been freely used in the preceding pages with a rather definite meaning, it would perhaps be unfortunate at present to attach a definition. The true sphericity, denoted by the Greek letter ψ is rather the term to be used and according to the definition given above.

ROUNDNESS

Wentworth dismisses my strictly mathematical expression and definition of roundness by the statement: "Roundness is a term which may properly be more loosely used, with reference both to solid shapes and plane figures. But when applied to a solid in the phrase 'maximum degree of roundness' most will agree that a perfect sphere is implied." A numerical value expressing the degree of a property must obviously be based on a sharp definition of the property. It is difficult to see the reason why Wentworth at this time advocates loose usage of the term "roundness," after years of endeavor to define roundness and to find a numerical value for the degree of this property. He said: "In studying this change in shape it is necessary to recognize varying degree of roundness, in other words, to have a numerical answer to the question of how round a given piece is."[25] Terms of loose usage should, of course, be avoided in scientific literature. If a term remains non-defined, it is permanently ambiguous.

The *Oxford* dictionary says that *roundness* is the quality of being round and that the meaning of *round* as a spherical or globular body

[25] "A Laboratory and Field Study of Cobble Abrasion," *loc. cit.*, p. 513.

is "somewhat rare." The following quotations give an idea of the general meaning of "round":

"The circumference or outer bounds of some circular objects; the complete circle of something . . . . a circle, ring or coil . . . . a circular part, form or arrangement of natural origin . . . . cylindrical, circular in respect to section; . . . . exhibiting a curvilinear form or outline; curved; forming a segment of a circle, etc."[26]

"Round. Circular, etc. F. rond, OF. roond, L. rotundus, from rota, wheel."[27]

"Rond (ron) Ronde lat. rotundus, de rota, roue ad. Qui a tous les points de la surface egalemente distance d'un point ou d'un axe central, soit sous une forme circulaire."[28]

Round as derived from the Latin *rota* ("wheel") does not mean spherical, and consequently a perfect sphere is not implied by the use of the term "maximum degree of roundness." A sphere is round because its circumference is round. Observing that a solid such as the moon is round, we are, however, not entitled to conclude that the moon also is a sphere. So far as its roundness is concerned, the moon may have any shape which in one section has a circular outline (cylinder, discus, cone, etc.).

According to the quotation from the *Oxford* dictionary, it is evident that *round* refers both to a circle and a segment of a circle. The correctness of this view is further shown by the use of such parasynthetic combinations as *round-backed*, *round-edged*, *round-cornered*, etc.

The formula suggested in my preliminary paper for expressing degree of roundness refers plainly to the roundness of the corners, yet it fits in perfectly with the idea that a circle has a maximum degree of roundness. The ratio of the radius of curvature of a circular outline (a circle) to the radius of curvature of the maximum inscribed circle (obviously the same circle) gives the numerical value 1, which is the value of the maximum degree of roundness. A cylinder terminated at each end by a half-sphere has also a maximum degree of roundness, because the ratio of the radius of curvature of its ends (corners) to the radius of curvature of the maximum inscribed circle in the given section equals the numerical value 1. Thus my formula

[26] *The Oxford English Dictionary.*

[27] Ernest Weekly, *An Etymological Dictionary of Modern English* (1921).

[28] Claude Augé, *Nouveau Larousse Illustré* (Dictionaire Universal Encyclopédique).

is an adequate expression for roundness in view of the common interpretation of the word "round." It fulfils the requirement that a circle has a maximum degree of roundness, and it can at the same time be used for expressing the degree of roundness of corners and edges and is therefore in agreement with the use of the parasynthetic combinations *round-cornered*, *round-edged*, etc.

Wentworth comments as follows on my statement that a cylinder terminated at each end by a half-sphere cannot become more rounded:

> If one accepts Wadell's definition of roundness in strictness, this is true but in the light of a more general concept of roundness such a statement is subject to challenge not only on geometrical grounds but also with reference to natural or experimental shape modifications.

Wentworth's arguments with reference to "natural or experimental shape modifications" will be discussed later. No indication is given of the reason why it is objectional on geometrical grounds, and a reply is therefore not possible. My roundness formula refers strictly to the corners and edges. That a solid has a circular outline in one cross section is, as we shall see later, rather a factor in the shape than in respect to the roundness. It is also to be noted that there is a difference between the roundness and the shape of a corner. As already pointed out in my preliminary article, the roundness is obtained by measurements of the radii of curvature in *one* plane. The total roundness of a solid may be obtained by measurements in three planes at right angles to each other; but one plane is generally satisfactory when dealing with quartz particles, provided that the measurements are undertaken in the largest plane parallel to the longest and intermediate diameters. The weakest part of an irregular quartz grain is as a rule along the direction of its smallest diameter. The results of fracturing, chipping, attrition, or solution of the particle are therefore most noticeable at right angles to the shortest diameter, i.e., in the plane of the longest and intermediate diameters. As we will see later, there are also other reasons for adopting this plane for measurements.

In hydraulics and gas dynamics an orifice is generally classified on the basis of the size and shape of the contracted area with speci-

fication in respect to sharp-edged and round-edged. Thus an orifice may be circular and round-edged or sharp-edged.

Stanton[29] found that the mean resultant pressure on dissimilar plates varied considerably for the same speed of a current of air, the mean pressure on a long narrow rectangular plate being nearly 60 per cent greater than that on a circular one. It should be noted that this difference is due to the difference in shape and not due to the difference between the degree of roundness of a rectangular and circular plate.

The shape of a plane figure may be expressed on the basis of the isoperimetric property of a circle[30] by the ratio:

$$\frac{c}{C} = \text{Degree of circularity},$$

where c is the circumference of a circle of the same area as the plane figure, and C is the actual circumference of the plane figure. The maximum value obtained by this formula is 1.000, which is the numerical value of the shape of a circle.

The difference between the degree of circularity (also the degree of sphericity) and the degree of roundness may now be illustrated. A circle has a maximum degree of circularity and also a maximum degree of roundness. A dodecagon is a closed plane figure composed of twelve straight sides and twelve plane corners, the radii of curvature of the former being infinitely large, and the radii of curvature of the latter equal zero. The degree of circularity of a dodecagon is 0.988, thus approaching very closely that of a circle, yet the degree of roundness of its corners is infinitely small or equals zero.

Plane figures could be constructed with still higher degree of circularity, yet with a roundness value of zero. The limit is reached in a perfect circle with an infinite number of "corners" joined together in a smooth circular outline with a maximum degree of roundness.

[29] T. E. Stanton, "On the Resistance of Plane Surfaces in a Uniform Current of Air," *Minutes of Proc. Inst. Civil Engineers*, Vol. CLVI (1903–4), pp. 78–139.

[30] C. Caratheodory und E. Study, "Zwei Beweise des Satzes, dass der Kreis unter allen Figuren gleichen Umfanges den grössten Inhalt hat," *Mathematische Annalen*, Vol. LXVIII (1910), pp. 133–40.

It follows that one of the essential parts of the roundness is a smooth outline.

In the practical solution of obtaining the roundness of the corners of a plane figure, corresponding to the projection area of a solid, various difficulties occurred. Any solid, no matter how smooth to the naked eye, will with increased magnification show unevenness of surface with corners and edges, and consequently also an uneven outline of the projection area. In order to obtain comparable values of the roundness, a standard size must be adopted. Large objects, such as boulders, must be reduced; and small ones, like sand grains, magnified to approximately the same size, i.e., the standard size, on which the measurements are performed. The average diameter of the standard size used for measurements of rock particles has been fixed at 70 mm., and microscopic particles have been enlarged to about that size by camera lucida or screen projection.

The following is intended to illustrate the practical results which under certain conditions may be obtained by the use of the roundness formula given in my preliminary paper. Assume that we have two cubes of sodium chloride, one with an edge of 70 mm. (approximately the standard size), the other with an edge of 1 mm. The solids are immersed in pure water. After a few minutes the smaller cube assumes a spherical shape, thus possessing a maximum degree of roundness. The corners of the larger cube have, during the same time, obtained approximately the same radius of curvature as the smaller, spherical particle; but the two solids would, of course, not be considered to have the same degree of roundness. According to the roundness formula, the sphere has a maximum degree of roundness, while the roundness value of the larger cube is very small.

If, on the other hand, the two cubes are subject to attrition by rolling without solution, the larger cube is more rapidly rounded than the smaller.[31] Thus, distinctly different results are obtained by the two processes—solution (in pure water) and attrition. Sorby made some observations bearing on these points. He said:

> In the specimens of decomposed granite which I have examined in greatest detail, the larger grains of quartz have a somewhat opaque surface, as if cor-

[31] No experiments have been made on attrition of cubes of sodium chloride. The outcome of such an experiment is inferred from the results obtained by attrition of solids of other composition. Some data are given later in the text.

roded, and the angles are rounded. This rounding is relatively much greater in the case of the smaller grains, which is the reverse of what is met with in worn sand. On the whole the facts seem to indicate that the quartz has been more or less corroded and dissolved by the action of the alkaline silicates set free by the decomposition of the felspar.[32]

The roundness of corners has a definite importance. Rounded corners offer under certain conditions less resistance than sharp ones in a current fluid. Rounding of sedimentary particles is a special type of disintegration attributed to attrition and sometimes to solution. Roundness is destroyed or diminished by fracturing and chipping, and a high degree of roundness is, therefore, often an indication of gentle conditions of wear relative to the size, hardness, and toughness of the particle. High degree of roundness is frequently the result of tractional[33] transportation of the particles, while sand grains carried in suspension are relatively unaffected by attrition.

THE PRACTICAL METHOD OF DETERMINING THE SPHERICITY VALUE OF QUARTZ PARTICLES

Wentworth asks what method I advocate for the measurement of surface areas to obtain valid averages with economy of time. There are several rather accurate methods used in connection with ore crushing and pulverization[34] and in physical chemistry.[35] None of

[32] H. C. Sorby, "On the Structure and Origin of Non-calcareous Stratified Rocks," *Quar. Jour. Geol. Soc.*, Vol. XXXVI (1880), Proc., pp. 48–92.

[33] For the meaning of traction see Gilbert, *op. cit.*

[34] Besides the references already given in this article, the following papers deal with surface determinations and related subjects.

P. R. v. Rittinger, *Aufbereitungskunde* (1867) (not seen); Karl v. Reytt, "Bestimmung des Arbeitsaufwandes zur Zerkleinerung der Aufbereitungsprodukte," *Oesterreichische Zeitschrift für Berg- und Hüttenwesen*, Vol. XXXVI (1888), pp. 229–31, 255–58, 268–70, 283–85; Ernest A. Hersam, "Economy of Power in Crushing Ore," *Mining and Scientific Press*, Vol. XCV, Part 2 (1907), pp. 621–26; Carl Naske, *Zerkleinerungsvorrichtungen und Mahlanlagen* (1921), pp. 1–13; Fritz Paneth und Walter Vorwerk, "Über eine Methode zur Bestimmung der Oberfläche adsorbierender Pulver," *Zeitschrift für Physikalische Chemie*, Vol. CI (1922), pp. 445–79; Hans Wolff, "Bestimmung der Oberffäche von Glaspulver," *Zeitschrift für angewandte Chemie*, Vol. XXXV (1922), pp. 138–40; W. A. Koehler and J. H. Mathews, "The Heat of Wetting of Lead Sulfate," *Jour. Amer. Chem. Soc.*, Vol. XLVI (1924), pp. 1158–83; Geoffrey Martin, "A Method of Accurately Determining Experimentally the Surface of Crushed Sand Par-

[Footnote continued on next page]

[35] E. K. Rideal, *An Introduction to Surface Chemistry* (1930); H. Freundlich, *Kapillarchemie* (1932); F.-V. v. Hahn, *Dispersoidanalyse* (1928).

these methods was adopted, because it was especially desirable for classification in the statistical charts to obtain sphericity values for each particle rather than bulk values for a great number of granules. A complete description of the method used will be given in a forthcoming paper. Only the principles are presented here.

For reasons which space does not permit to discuss here, the maximum projection area was used as a base for sphericity determinations.[36] It should be noted, however, that the most desirable value is that of the degree of true sphericity as obtained by the formula previously given. For quartz particles the value of the degree of true sphericity was found to agree reasonably well with the value obtained by the practical formula given below.

To obtain the shape of quartz grains, the particles were placed under the microscope. The slide was given a few gentle taps with a pencil so that all particles would come to rest on one of their largest surfaces, parallel to the longest and intermediate diameters. This gives approximately the largest projection area. The standard size was obtained by the use of the proper objective for each grade size, and by regulating the distance between the camera lucida mirror and the drawing paper. The area of the reproduced particle

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ticles," *Trans. Ceramic Soc.*, Vol. XXV (1925–26), pp. 51–62; J. Gross, S. R. Zimmerley, S. J. Swainson, "Surface Measurement on Finely Ground Particles and Its Relationship to the Crushing Laws," *Bull. Univ. Utah*, Vol. XVI, No. 4 (1925), pp. 57–76; Herbert F. Kriege, "Relation between the Fineness of Limestone and Its Rate of Solution," *Rock Products*, Vol. XXIV (1926), pp. 65–68 (not seen); W. A. Koehler, "A Method for the Determination of the Relative Surface Areas of Powdered Materials," *Jour. Amer. Ceramic Soc.*, Vol. IX (1926), pp. 437–43; J. Gross and S. R. Zimmerley, "Crushing and Grinding. I. Surface Measurement of Quartz Particles," *Amer. Inst. Min. and Metall. Engineers, Tech. Pub. No. 46* (1928), pp. 1–16; Fr. W. Freise, "Untersuchung von Gesteinen auf Abnutzbarkeit bei Verfrachtung im Wasser," *Mineralogische und Petrographische Mitteilungen*, Bd. XLII (1932), pp. 48–58.

[36] In respect to roundness determinations a reason has already been given on p. 320.

In respect to sphericity determinations it should be noted that, as the shape departs from that of a sphere, i.e., with decreasing degree of sphericity, an increase usually takes place of (1) the longest diameter, (2) the largest cross-sectional area, (3) the surface of the solid, and (4) the sum of the projection areas of the solid in different positions. On the other hand, the degree of circularity of the different cross-sectional areas of the solid decreases with decreasing degree of sphericity. All these factors influence the resistance, exerted by the fluid on the particle in translation in different positions. The coefficient of resistance or drag as a function of *Reynolds number* is therefore influenced by the shape of the particle. The author will return to this question in a special paper.

was measured with a polar planimeter. The following formula was adopted for obtaining the sphericity value:

$$\frac{\delta}{\Delta} = \text{Degree of sphericity},$$

where δ is the diameter of a circle of an area equal to the area obtained in the standard size by projecting the grain at rest on one of its surfaces parallel to the plane of the longest and intermediate diameters of the particle, and where Δ is the diameter of the smallest circle that will circumscribe the grain projection. The sphericity value achieved by this method is denoted by the Greek small letter ϕ.

Space does not permit here any arguments for this method. Details will be given in a forthcoming paper. Reasons for reproduction and projection of the plane parallel to the longest and intermediate diameters has been given in the preceding chapters both in respect to roundness and sphericity. The method applies only to quartz grains. For other minerals other methods have been suggested. In the course of the work various new ideas have occurred, and further experimental studies are planned.

My diagrams are based not on any averages but on a reasonably accurate determination of the *true* sphericity for each quartz particle. It is probable that the error due to the formula given above in general does not exceed one class-interval. The classes used range from 0–.05, .05–.10, .10–.15, .15–.20, and so forth, up to .95–1.00. The accuracy of the determinations was to some extent checked by taking the average sphericity value for each sieve grade size and comparing it with Lamar's[37] results, which are based on porosity determinations with a device, essentially of the type used for dispersion measurement on the basis of pore space. My average values for grade sizes of St. Peter sand grains were practically identical with those obtained by Lamar by an entirely different method.

MODIFICATION OF SHAPE AND ROUNDNESS OF ROCK PARTICLES BY NON-CHEMICAL FORCES

The shape of rock particles may depend on a variety of factors. One is the difference in cohesive forces in different directions. Owing to the pronounced cleavage of mica, this mineral is never found in

[37] *Op. cit.*

the shape of a sphere. The mode of crushing influences, in all probability, the average shape of particles. It is reasonable to assume that different results are produced by grinding and by direct blows. The shape is influenced by the crystal structure, inequalities of expansion, inclusions, etc. Beckenkamp[38] said:

> Ein Zerreissen eines Körpers tritt dann ein, wenn die (etwa durch das Einpressen einer Messerschneide hervorgerufene) Deformation einen solchen Betrag erreicht, dass die inneren attraktiven Kräfte den inneren repulsiven und den äusseren Kräften nicht mehr das Gleichgewicht halten können. Es tritt demnach Spaltung senkrecht zu solchen Richtungen ein, für welche die eine derartige Deformation erzeugenden äusseren Kräfte einen minimalen Wert haben.

It is also likely that, because of capillary action, different shapes are produced by disintegration in air and water. This tentative conclusion is based upon the method of making thin laminae of gypsum and mica for optical purposes.[39] Much thinner laminae are produced by splitting the mica in water than in air. Should experimental results show that micas by transportation and disintegration in water break up in thinner flakes, thus achieving a lower sphericity, than by disintegration in air, a distinction between eolian and aqueous deposits on this basis will eventually be possible.

In the present issue of the *Journal*, Wentworth expresses his opinion that the result of wear is ". . . . due in part to the decrease in ratio of supporting volume to area of attacked surface." He should not draw any conclusion of this kind from his experimental results. He simply weighed the solids at certain intervals and calculated the distance covered by the revolving drum. The surface areas produced in the course of attrition were never measured.[40] He also

[38] J. Beckenkamp, "Atomanordnung und Spaltbarkeit," *Zeitschrift für Kristallographie*, Vol. LVIII (1923), p. 17.

[39] J. Hirschwald, *Handbuch der bautechnischen Gesteinsprüfung* (1911), p. 275.

[40] On p. 514 of his paper "A Laboratory and Field Study of Cobble Abrasion," Wentworth makes the following statement: "In (1) the measurement of surface area with sufficient accuracy, while not impossible, was too laborious to use with more than one or two specimens." The method used and the result obtained were never published. It is evident that no systematic measurements were undertaken, because on p. 511 of the same paper Wentworth says in respect to wear: "This is probably in part due to the increasing ratio of surface to weight." It is evident that Wentworth has actually no results to fall back on when he criticizes my views as "objectional to natural or experimental shape modifications."

neglected to consider that as the solids are reduced in size their circumferences are also reduced, with the results that the number of revolutions of the solids per mile is increased.

When a solid of cubic shape is reduced in size by attrition in a revolving drum, the volume decrease does not necessarily have any definite proportion to the surface decrease. The change in surface area depends on the change of shape and the decreasing volume. If a cube of 1 cubic decimeter and a surface area of 6 square decimeters (specific surface 6) is reduced in size without change in shape to a cube of 0.5236 cubic decimeters, the surface of the smaller cube amounts to 3.9878 square decimeters and the specific surface is increased to $\frac{3.9878}{0.5236} = 7.44$. If the original cube, however, is reduced in size to 0.5236 cubic decimeters (the volume of the inscribed sphere) and at the same time undergoes a change in shape to a globe, the produced sphere has a surface of 3.1416 square decimeters and a specific surface of 6. Thus the surface-volume ratio is the same as for the original cube. When a cube is reduced in size by attrition in a drum, the produced sphere is, of course, actually smaller than the sphere inscribed in the original cube; but the example shows that the change in shape must be taken into consideration in an experiment of the kind conducted by Wentworth.

My views on the factors involved in attrition of a solid in a revolving drum are different from those of Wentworth. It is perhaps true that the rate of attrition and loss of volume is influenced, as in the case of solution, by an increasing ratio of surface to volume. Thus the shape may eventually influence the rate of attrition. In Wentworth's discussion in the present issue of the *Journal* the problem is, however, to what extent the attrition influences the shape—an entirely different question. The rate of attrition may be influenced by the shape, but the shape is determined primarily by the properties of the particle and secondarily by the rigor of attrition.

Assume that a cube, subject to attrition in a revolving drum, is composed of numerous small spheres arranged according to the rule for maximum pore space or open packing, so that each sphere has six contacts with the surrounding spheres. Assume, also, that the

cohesional[41] forces between the spheres are equal in all contacts. The spheres situated in each apex of the solid corners are bound to the mother-cube by three contacts; those in the edges by four contacts; while those in the plane faces are tied to the mother-cube by five contacts. It is therefore to be expected that the solid corners offer the least resistance to attrition, the edges more, and the plane faces most. By removing one sphere from the apex of a solid corner, three other spheres are exposed, each tied to the mother-cube by only three contacts. We may then expect that these three units may be removed with the same ease as the first sphere. Continuing the removal of spheres as they are successively exposed with three contacts, the mother-cube gradually shrinks in a linear direction from the apex of the corners toward the center of the edges, from the center of the edges toward the center of the plane faces as well as toward the center of the cube, and finally also from the center of the plane faces toward the center of the cube. The volumetric loss is greatest in the corners, and the linear centripetal shrinkage is greater than in the edges and plane faces. The final result is a sphere. When this form has been reached, the solid maintains this shape, because the formation of a corner would immediately result in the detachment of the minute sphere so situated.

If, on the other hand, the cohesion between the minute spheres in their contacts with each other is different in different directions, the magnitude of these cohesional forces and the rigor of the attrition will determine whether a sphere or some other shape is produced. If the attrition is feeble, the solid will tend to shape itself according

[41] For the sake of the example the term "cohesion" is here used in a broader sense, meaning the bonds which tend to keep the minute parts together. In actuality we have both attractive and repulsive forces. Beckenkamp (*op. cit.*) said: "Der Widerstand gegen die Trennung der Atome hat einen minimalen Wert, wenn die auf auf die Elektronen zurückzuführenden Kräfte einen minimalen attraktiven oder einen maximalen repulsiven Wert haben."

Huggins gave the following rules governing crystal cleavage: "(1) Cleavage tends to occur so as to leave the two new crystal surfaces electrically neutral. (2) Where some of the bonds in a crystal are weaker than others, cleavage will take place in such a way as to rupture the weaker bonds in preference to the stronger ones. (3) All bonds being equally strong, cleavage will occur between the planes connected by the fewest bonds per unit area (of the cleavage plane)."

Maurice L. Huggins, "Crystal Cleavage and Crystal Structure," *Amer. Jour. Sci.*, 5th ser., Vol. V (1923), pp. 303–13.

to the difference in the cohesional forces within; in other words, those spheres are most readily removed which are so situated that they are bound to the mother-solid by weaker forces. It is evident that some spheres bound to the solid by stronger forces are also removed as they are exposed, according to the views expressed in the preceding paragraph. The total result, however, is that the final shape is determined by the difference in the cohesional forces within the solid.

Wentworth's own experimental work gives an example of the views expressed above. He found that when the marble cubes were rounded and reduced to a weight of 3.29 grams by attrition in the partially water-filled drum, they began to decrease in roundness and apparently also in the degree of sphericity. Wentworth explained this by the durability, as he called it, along various axes. He said that for any given rock there is an ellipsoid of equilibrium for wear by abrasion which depends on the durability along the axes. For a perfectly homogeneous rock this ellipsoid is a sphere, as Wentworth expressed it. For a non-homogeneous rock it is an ellipsoid of greater or less eccentricity. The hypothesis advanced by Wentworth is that the eccentricity is greater for smaller than for larger particles. He said:

> Thus it is conceived that at 3.29 grams the marble cobbles had nearly or quite reached the ellipsoid of durability for that size and from then on were practically following the equilibrium figure in its decreasing values of R as the size grew less.

The hypothesis actually does not explain anything, since the principal question is left open, namely, why the eccentricity is greater for smaller than for larger particles. Wentworth did not consider that he had water in the revolving drum. As long as the solids were sufficiently large and heavy, the slight difference in the cohesional forces within the marble could not be reflected in the shape, because the weight and the momentum of the marble was great enough to eliminate any slight difference in cohesion. In other words, the minute particles constituting the surface were rubbed away by attrition irrespective of a slight difference in cohesion in different directions. As the marble approached the weight of 3.29 grams, the water current produced in and by the revolving drum buoyed up

the marble somewhat so as to make the impacts less severe. As the weight was further reduced, the attrition became so feeble that the minute parts of weaker cohesion were more readily detached from the marble than those bound by stronger cohesion. The solid tended to shape itself according to the difference in the cohesional forces within. Thus Wentworth's results find a simple explanation on the basis of the views set forth in this paper.

MODIFICATION OF SHAPE AND ROUNDNESS OF ROCK PARTICLES BY SOLUTION

Wentworth's ideas on shape modification by solution are too generalized, and lack in many cases experimental support. If a process of solution consists of a number of successive stages, the velocity of the whole process depends on that of the slowest component stage. The rate of diffusion is a part of the process of solution, but it is not necessarily the determinant factor in the rate of solution. It seems to me that if the diffusion (without stirring of the solvent) has anything to do with the change of shape of particles subjected to solution, such changes may eventually be due to difference in the density of the diffusion layer in the corners, edges, and plane faces. Even this, however, is rather uncertain in view of experimental results. It is well known that the area of the attacked surface usually has been found to influence the rate of solution, but the influence of the solution process in shaping the solid is a different question. While the volume and the surface area are generally decreasing till the solid is dissolved (the specific surface is increased), the shape usually soon takes a definite character[42] (*Endkörper*) which under constant conditions is maintained to the end of the process.

[42] F. Becke, "Aetzversuche am Fluorit," *Tschermak's Mineralogische und Petrographische Mitteilungen*, Vol. XI (1890), pp. 349–437; G. Wulff, "Zur Frage der Geschwindigkeit des Wachsthums und der Auflösung der Krystallflächen," *Zeitschrift für Krystallographie und Mineralogie*, Vol. XXXIV (1901), pp. 449–530; V. Rosicky, "Über die Symmetrie des Steinsalzes," *Beiträge zur Krystallographie und Mineralogie*, Vol. I (1914–18), pp. 241–56; O. Meyer and S. L. Penfield, "Results Obtained by Etching a Sphere and Crystals of Quartz," *Trans. Conn. Acad.*, Vol. VIII (1889), pp. 158–65; S. L. Penfield and L. V. Pirson, *Contributions to Mineralogy and Petrography*, pp. 160–67; W. Poppe, "Über die Auflösung, von Natriumchlorid- und von Natriumchlorat-Kristallen," *Neues Jahrbuch für Min. Geol. und Paleon.*, Vol. XXXVIII (1915), Beilage-Band, pp. 363–428; H. Bauhansund V. Goldschmidt, "Über Endkörper und Lös-

In the modification of shape and roundness by solution, the result depends mainly upon three factors and their influence on the process. These factors are: (1) the properties of the dissolving particle, (2) the properties of the solvent, and (3) currents and eddies[43] produced in the solvent by the process of solution itself or by stirring of the fluid.

Space limitation does not permit further discussion on the subject. The facts remain that, under certain conditions, spherical or highly spherical and well-rounded solids are produced without stirring of the solvent, while in other cases a decrease in both roundness and sphericity takes place, probably due to the atomic arrangement and the cohesional forces within the solid and a weaker interaction between solute and solvent. If the solid has a fixed position and strong solution currents are present or artificially produced, then the shape modification is influenced by the current velocity and eddies produced about the solid, especially on its lee side. Finally if a number of sand grains in a solvent are stirred around in the bottom of the vessel, their shape is to a large extent a result of attrition. The last is important to have in mind in experimental shape modification of rock particles in solution.

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ungsschwindigkeit von Flussspat," *Beiträge zur Krystallographie und Mineralogie*, Vol. I (1914–18), pp. 219–40; J. J. Galloway, "The Rounding of Grains of Sand by Solution," *Amer. Jour. Sci.*, 4th ser., Vol. XLVII (1919), pp. 270–80; J. J. P. Valeton, "Wachstum und Auflösung der Kristalle," *Zeitschrift für Kristallographie*, Vol. LIX (1924), pp. 135–69, 335–65; Vol. LX, pp. 1–38; G. Aminoff, "Versuche über Verdampfung von Kristallen," *Zeitschrift für Kristallograpie*, Vol. LXI (1925), pp. 373–79.

[43] P. Rosin und H.-G. Kayser, *op. cit.*

REVIEWS

Earth History. By LUTHER C. SNIDER. "The Century Earth Science Series," edited by Kirtley F. Mather. New York: Century Co., 1932. Pp. xiii+683; figs. 335.

"Even Julius Caesar could not tell his fellow-Romans about his wars with the Gauls without first making them acquainted with the tripartite division of Gaul, and with a few [other] fundamental facts," observes Dr. Snider, who then explains that similarly "the first section" of his new and novel *Earth History* "contains brief synopses of the essential information on which earth history is based." But other than in this momentary and partial conformity to orthodoxy in geological textbook style, the treatment of the subject differs so radically from those usually employed that the author may be congratulated on having sponsored one of those rarities—"something new under the sun."

The principal innovations are: (1) the reduction of material introductory to historical geology to a minimum; (2) the use of "natural cycles" as a basis for the division of earth history instead of the "arbitrary eras and periods"; (3) the separation of the history of "living things" from the physical record; and (4) the consideration of the pre-Cambrian, and theories of earth origin, subsequent to the discussion of the better-known geological periods. Of course, other writers have made some little use of one or another of these various devices, but none heretofore has employed them so sweepingly or in such combination. As a result many of the difficulties in the way of a satisfactory presentation of earth history are overcome. When the story is told in the conventional chronological way the account of necessity begins with the most speculative phases of the record; but when the tale proceeds from the better-understood events to the more or less indefinitely known sequences of early earth stages, the time element is difficult to handle inasmuch as results must be considered before their causes are mentioned. But Dr. Snider has cut these Gordian knots by linking all events, both physical and organic, into the inexorable march of time. Events described, the underlying principles are explained; facts once presented, the theories advanced to account for them are discussed; and fine distinctions are drawn between mere working hypotheses and generally accepted principles.

The book, which is dedicated with fitting tribute to Thomas Chrowder Chamberlin, is divided into four main sections. The first, which is titled "The Book of the Earth," discusses (1) "Earth Materials and Processes," (2) "Keys to Earth History," (3) "The Earth Time Scale," (4) "Cycles of Earth History," and (5) "Borderlands, Basins, and Troughs." Section II, the "History of the Continents," is composed of chapters on (1) "The Early Paleozoic Cycle," (2) "The Caledonian Revolution," (3) "The Late Paleozoic Cycle," (4) "The Appalachian Revolution," (5) "The Mesozoic Era," (6) "The Cenozoic Revolution," (7) "The Pleistocene Glaciation," (8) "Pre-Cambrian Time," (9) "Theories of Earth Origin," (10) "Theories of Mountain Making," and (11) "Theories of Climate." Section III, "The History of Living Things," is, of course, in large part the record of *dead* things. It is divided in orthodox fashion. Section IV, "Man and Earth History," considers (1) "Man and Minerals," (2) "Earth Tragedies," (3) "The Meaning of Scenery," (4) "Development of Earth Sciences," (5) "The Present Aspect of the Continents," and (6) "Present and Future."

This review of chapter headings gives some idea of the great scope of this volume. All of the topics are discussed lucidly and succinctly in a style that is always clear and, in many passages, beautiful. The geologist and lay reader alike will be intrigued by the more than 300 excellent illustrations, most of which have been obtained from various commercial concerns and consequently are not the "old favorites" which stare at one from the pages of practically all geology texts. It is doubtful, however, if the average layman will, despite the clarity of discussion, be able to wade painlessly through the nearly 700 pages. Moreover, the all-too-numerous disciples of that famous professor who boasted that he had not "changed a word of his class notes in twenty years" probably will be slow in adopting this radically different, but seemingly very teachable text.

The volume is particularly up to date in that it speaks fluently of 1931's earthquakes, landslides, tornadoes, and scientific thought. The detailed discussion of the nebular hypothesis, however, is perhaps unnecessary chewing of old fat; and the author is remarkably conservative on the subject of late Paleozoic glaciation, particularly as far as the map on page 123 is concerned. But most of the many maps are well done, although some, notably the one on page 173, carry the exaggerations, to some extent necessary because of the scale, entirely too far, and the one showing the extent of Pennsylvanian seas is very inaccurate. Moreover, the author in some instances has almost as much trouble with his "natural cycles" as others have encountered with the "arbitrary periods and

eras"; but there is no doubt that the concept of major cycles is one which is recommended not alone by its factual bases, but by its teachability as well. It is to be hoped that the later volumes of the "Century Earth Science Series," under the able editorship of Dr. Mather, will be as excellent as the first.

The final chapter of *Earth History* concludes with a statement to which most geologists will subscribe:

> If man can persist *as such* through a complete earth cycle, to say nothing of the five or six cycles which should be comprised in a billion years . . . . his mental equipment will have enabled him to overcome the difficulties of his physical environment and of the biological laws which have brought an end to [other] species; [and] he will be, by whatever means created, a special creation of a special kind.

CAREY CRONEIS

The Geology of the Tsinlingshan and Szechuan. By Y. T. CHAO and T. K. HUANG. (Geological Memoir No. 9.) Peiping: The Geological Survey of China, 1931. Pp. 228; figs. 45; pls. 19.

Far Western China is not only of surpassing beauty, natural and cultural, but the eastern approaches to the great orographic plexus of Central Asia are of much geologic significance. A large tract of dominantly mountainous country, from the Wei Ho to the Yangtze Kiang, has been covered in this report. The field work was necessarily of a rapid reconnaissance nature carried out by both authors, but the written report is the work of the junior author after the tragic death of his colleague.

The Tsinlingshan, sometimes called the backbone of China proper, is the name applied to the whole mountain mass which strikes east and west from southern Kansu province to the borders of Honan. Farther south is the Tapashan range, which, from its western end, is continued southwestward in a lofty mountain complex buttressing the great Tibetan uplift. Immediately southeast of the southwest-trending range is the striking Red basin of Szechuan, one of the loveliest garden spots on the face of the globe.

The northern Tsinlingshan, composed of pre-Cambrian and early Paleozoic rocks, acquired its structure during the Hercynian deformation. The southern Tsinlingshan, composed of Paleozoic and Mesozoic strata, escaped the Hercynian movement, but its sediments were strongly folded, intruded by granites, and metamorphosed during the Yenshanian diastrophism near the close of the Jurassic. The active thrusting was toward the south. The Tapashan was less strongly folded and granite intrusions have not been recognized in that area. At the end of the Cretaceous, def-

ormation again occurred along the same structural lines and in addition flexed the red Cretaceous strata of the neighboring Red basin into gentle anticlines. The larger picture is that of a southward migration of geosynclines followed, in successive steps, by southward migration of the belts of active folding. Between the Eocene and the Quaternary the ranges were further uplifted, accompanied by normal faulting.

In general, the dominant Jurasside orogeny with granite intrusions, followed by Cretacide folding and strong uplifting in the late Tertiary, calls to mind somewhat similar manifestations on the American side of the Pacific. A new discovery was evidence of a Caledonian disturbance in northwestern Szechuan. Such had not previously been recognized in Central China.

The treatment of the stratigraphy is very commendable and the stratigraphic column is of much interest. A separate atlas of large maps accompanies the report.

R. T. C.

Contributions in Geology and Geography. Edited by LEWIS F. THOMAS. (Washington University Studies, Science and Technology, No. 7.) St. Louis, 1932. Pp. 122.

This volume includes the following contributions: "The Geographical Landscape of Metropolitan St. Louis," by Lewis F. Thomas; "The Geologic Work of Ice Jams in Sub-Arctic Rivers," by Chester K. Wentworth; "The Opemiska Granite Intrusive, Quebec," by Carl Tolman; and "Synonomy of the Mid-Devonian Rugose Corals of the Falls of the Ohio," by Courtney Werner.

R. T. C.

Symposium on Weathering Characteristics of Masonry Materials. Vol. 31. Philadelphia: The American Society for Testing Materials (1315 Spruce Street), 1931. Pp. 128. $.60.

Many phases of this subject, which borders on geology, are extensively discussed. The weathering of natural building stones and of slate will interest geologists.

R. T. C.

Mineral Economics. Lectures under the Auspices of the Brookings Institution. "A.I.M.E. Series." New York: McGraw-Hill Book Co., Inc. Pp. 311; figs. 31. $2.50.

This book records a series of lectures delivered under the auspices of the Brookings Institution of Washington, D.C., by fifteen well-known

geologists, mining engineers, and mineral economists, among them the editors of the volume, F. G. Tryon and E. C. Eckel. The volume opens with a masterly outline of the field of mineral economics by F. G. Tryon and F. E. Berquist in which especial interest attaches to the analysis of the trend of world-prices indicating that in general the effects of exhaustion of mineral resources have been more than balanced by other factors such as improvements in mining, treatment, and transportation. Interesting comparisons between the output per worker in the United States and in certain European centers are used as indicators of the success with which technology and its allies are coping with the increasing natural handicaps of mining.

Chapter ii, by John W. Frey, dealing with the geographic distribution of mineral production, contains an excellent series of world production maps. "Cycles of Mineral Production" forms the title of a chapter by D. F. Hewett. Without listing all the chapter headings mention may be made of that by H. Foster Bain on "The Rise of Scrap Metals"; "Tariffs and Exhaustible Resources," by J. W. Furness; "Mineral Exploration and Discovery," by C. K. Leith; "Production Control in the Petroleum Industry," by Leonard Logan; and "Precious Metal Supplies and the Price Level," by G. F. Loughlin.

The entire volume is of outstanding interest and value to geologists and economists.

E. S. B.

Internationaler Geologen und Mineralogen Kalender, 1933–34. By R. CRAMER. Stuttgart: F. Enke. RM. 8. Pp. 408.

This unique geological address book in up-to-date form is a valuable addition to the library of any geologist or geographer. There are 254 pages of names and addresses in alphabetical arrangement, followed by 49 pages of data on geological surveys (arranged by continents and countries), 77 pages on universities, technical colleges, and leading museums, and 28 pages on geological societies. Other material covered in the preceding (1925) edition is omitted. Such a compilation cannot be expected to be without omissions and errors (such as referring to the Geological Society of America as the Southwestern Geological Society, on p. 404). Nevertheless, the work is so very useful that its continued publication at frequent intervals should be assured by a widespread sale of this edition.

D. J. F.